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Response to rebuttals of Beyea2025 by Cardarelli and Calabrese.

A defense of my article in *Health Physics*, entitled:

"False and misleading claims of scientific misconduct in early research into radiation dose-response: Part 1. overlooking key historical text"

Health Physics 128: 507–523; 2025.[1]

which is available here.

(This report is written in response to rebuttals by John Cardarelli (Cardarelli 2026)[2] and Edward Calabrese (Calabrese 2026).[3])

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[1] https://journals.lww.com/health-physics/fulltext/2025/06000/false_and_misleading_claims_of_scientific.9.aspx

[2] Cardarelli JI. False and Misleading Statements Leading to an Unreliable Source of Information Regarding Early Research into Radiation Dose-Response: Part 1. A Response to Beyea. Health Physics 130: 434–455; 2026.

[3] Calabrese EJ. Confessions of an LNT Heretic: What Happens When You Challenge Biomedical and Environmental Dogma: Rebuttal of Beyea Commentary 2024. Ibid.: 456–500.

Abstract: Calabrese and Cardarelli have responded separately to my 2025 paper in *Health Physics*, where I showed that Calabrese overlooked key historical text, leading to false premises on which he based charges of scientific misconduct on the part of early geneticists. Not allowed by the editor to reply to the rebuttals directly in the journal, I do so here through this web-based document. Both of their rebuttals focus on concerns expressed by early radiation geneticists in selected correspondence, transcripts and reports, while ignoring the resolution of those concerns in later primary documents, such as final reports, which they overlook. A prime example is the failure to acknowledge the seven places in the 1956 NAS genetics report, where uncertainty and limitations in risk calculations were acknowledged. Language was included to cover the views of all those committee members who had expressed reservations before finalization, which is standard policy for NAS reports. No scientific misconduct coverup occurred. Calabrese and Cardarelli failed to account for chronology. Other examples are covered in this reply. As amateur historians, they try to define their way out of the problem by saying that the documents I use, which are both primary and secondary, are not as good as theirs, and that professional historians, on whom I relied in a few cases, made errors or were incompetent. The arrogance is unbounded. Calabrese cannot even get the definition of primary documents correct, confusing them with privately available documents, yet he claims that I have not performed professional historical research. As if he would know, having never cited any definition nor had a named historian review his work, as I have done. The bulk of the 66 journal pages penned by Calabrese and Cardarelli amounts to diversion from my allegations of overlooking key text. Nevertheless, I respond to all of their distractions in a general introduction and separate annotations of their papers. I also include an Errata, and I defend two modern historians. I make use of newly created raw transcripts to rebut Cardarelli's written interpretation of contested segments of the video series he prepared about Calabrese. Note that Part 2 of my "False and Misleading" series defends additional geneticists and rebuts Calabrese's misconduct claims made in post video papers published through mid-2025.

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Introduction and summary

The Health Physics Society in 2022 under the then presidency of John Cardarelli posted online a 22-part video series featuring the scientific misconduct theories of Edward J. Calabrese, an emeritus professor at the University of Massachusetts. This posting was followed by an article in *Health Physics* written by John Cardarelli and other leaders of the Health Physics Society (Cardarelli et al. 2023) in which Calabrese's claims of misconduct by past radiation researchers were treated as authoritative and trustworthy. I responded critically to that article in my Part 1 Forum article, arguing that Calabrese's overlooking of key sections of historical text led him to false premises that seemingly made plausible his claims of scientific misconduct (Beyea 2025 at p. 508). Cardarelli and Calabrese wrote separate long rebuttals of 22 and 44 journal pages, respectively (Calabrese 2026, Cardarelli 2026) that repeated Calabrese original arguments as truth without addressing the overlooked text and my allegations of false premises.

Unfortunately, I was not allowed by the editor to respond to those rebuttals, given the editor's desire to move on and let readers decide.[4] This produced a very unbalanced situation. Calabrese got a video series, an endorsing article by Cardarelli et al. and an opportunity to rebut my Part 1 article, without any chance for me to respond to it in the journal, even briefly, which makes it look like I have no response. My limited option is to respond with a web-based document to show for the historical record that 1) both Cardarelli and Calabrese's rebuttals are distractions from Calabrese's false premises, 2) my Part 1 allegations of false premises have gone unchallenged, and 3) criticisms of me in the rebuttals are meritless with a few exceptions, which I herein address. I hope my

[4] Brant Ulsh, personal communication, 23 January 2026.

analysis will be helpful for science historians and future NAS committees, as well as any diehard reader who should happen to come across this document. In addition to this introduction and summary, I have attached appendices that go section by section through the Cardarelli and Calabrese rebuttals.

Calabrese asks why I did not note some of his post-2022 papers. These I cover in my Part 2 Forum article, where I expand the number of early researchers that I defend. Most significantly, I provide defenses of Edward B. Lewis and William L. Russell. I also cover the so-called J-shaped response in early leukemia data, primarily related to Edward Lewis.[5] And I cover papers that Calabrese published after the 2022 video series through mid-2025. I could not do all this within one paper, which is why I split my critiques into multiple parts, but Calabrese should rest assured that I am trying to keep up with his prolific pen.[6]

As stated above, in their rebuttals of Part 1. neither Calabrese nor Cardarelli face up to my allegations of Calabrese overlooking key historical text and deriving false premises. This is prime evidence that they have no real rebuttal to make. Instead, they

[5] New information: The dips that Calabrese sees in the two early papers available to Lewis, each analyzing basically the same data, were based on 1 count in the next-to-lowest category of distance from the bomb's center. There was a second count that Calabrese did not use without explanation, presumably because it was for a case where the exposed person had symptoms of high radiation dose. However, there is no dip if both leukemia counts are included. Why Lewis would have ignored the second count was not explained by Calabrese. True, the appearance of such symptoms was deemed "fishy" by a member of the 1956 NAS pathology committee in the transcript of their January 1956 meeting NAS. Transcript of STUDY GROUP ON PATHOLOGICAL EFFECTS OF ATOMIC RADIATION, Vol 1.; 1956b. but the Chairman, Shields Warren, noted on p. 44 that some symptoms of excessive radiation exposure could be mimicked by poor health conditions that occurred after the bombing. The labeling of cases by symptoms was dropped in leukemia studies after the first two, those available to Lewis before his 1957 paper.

[6] My goal in my Calabrese critiques has been to provide a defense of early genetic researchers against what I have found to be false charges of scientific misconduct made by Calabrese and repeated by Cardarelli. I have not analyzed Calabrese papers for problems unrelated to his denigration of past researchers.

engage in distraction, 1) repeating Calabrese's original arguments as truth, 2) contesting points not relevant to the alleged false premises, 3) criticizing my earlier papers in 2016 and 2017, regardless of any role they actually play in my 2025 paper, 4) trying to claim that Calabrese's use of early correspondence insulates him from criticism,[7] and 5) that I do not use primary documents, only secondary ones. Calabrese is a toxicologist, not a historian, which may explain his mix-up about primary versus secondary sources. He cites no definitions or references from historians or historical societies. His work is not reviewed by historians. In contrast, I consulted with a science historian who has written about this period and who reviewed my Part 1 before submission, without any complaint about primary vs. secondary sources.

In this reply, I repeatedly make use of quotations from correspondence that contradict Calabrese's claims based on his selection and interpretation of this type of information. I do so reluctantly as an amateur science historian not trained to handle correspondence,[8] worried that I am in some way validating Calabrese's selective use of correspondence. However, I cannot let claims of superiority of primary-sourced evidence like correspondence stand without demonstrating that Calabrese's "selective stakeholder

[7] as well as . . Their defense is not to address the overlooked text, but to say that Calabrese uses primary documents which somehow inoculates him from having overlooked key Text in documents they criticize, which ironically happen to be primary documents, namely early scientific papers and reports.

[8] As I said in 2017: "I am not a historian, either. When I go outside the scientific record to rebut trans-science charges like fraud, misconduct and financial self-interest, I try to rely on secondary materials prepared by historians, which often include quotations from, and analysis of, private correspondence and/or meeting transcripts. Only when the scientific and historical literature are insufficient to reply to an accusation –only then will I discuss or cite to my own readings of the transcripts of the two 1956 NAS committee meetings or to quotations from selected private correspondence that have appeared in earlier Calabrese publications. I particularly want to avoid the approach of postulating the thought processes of historical figures, which can look like an attempt to pressure the reader into a favorable frame of mind. I also want to avoid the risk of missing cultural and historical context. (SeeSupplementary material, S-8.3).

perception"[9] is also at work here, as was the case with reports and articles. In this introduction, I focus on what I deem are the most important counterclaims made or omitted by Calabrese and Cardarelli, including those that attempt to counter my analysis of his main charges of misconduct on the part of the 1956 NAS genetics committee. Counterclaims not covered in this introduction are dealt with in separate annotations of each rebuttal. I annotate the rebuttals of Cardarelli and Calabrese to check if they have found any mistakes I might have made and for possible use by science historians.

In this response I quote from unofficial transcripts that I made of episodes of the video series using modern software, specifically, *Otter.ai*.[10] I do this to respond to claims that I did not properly reflect in Part 1 the actual content of the video series. Cardarelli decided not to produce an official transcript, which was the major reason I have tried in my Forum articles to tie all my claims about Calabrese's views to his journal articles. I determined early on that almost all claims made in the video series by Calabrese are based on his written works, although there may be some exceptions.

The absence of a transcript allows claims to be made about what was said in the series depending on what the viewer heard, interpreted, or recalled. Based on Cardarelli's claims about video content made in his rebuttal to Part 1, there must have been two video series, the one he saw and the one I saw. Here, I use the raw transcript to check my account.

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[9] From my Part 1: "By the coined phrase "selective stakeholder perception," I mean the inability of some advocates to see words in documents that contradict their thesis. It is common. I have a bit of it myself. It is not a moral failing, which is why I appropriate a psychological term, "selective perception," for the wording. Some of it will inevitably go uncaught but can be minimized by rereading key documents as a final check from the perspective of someone who takes a different side. If it occurs repeatedly, an author's work can be one-sided and misleading."

[10] Because these transcripts are raw and have not been corrected by speakers, they will inevitably have some errors.

Finally, I note that I believe that both Cardarelli and Calabrese are doing what they think is best for society, and that they have a role to play in keeping no-threshold thinking from becoming dogma. They have both written excellent and objective articles. That said, this has not always been the case, as I wrote in Part 1 and as I now point out anew. My hope is that they will eventually correct their errors about misconduct and move on. There will be plenty of material left for critiques of LNT's development.

Calabrese and Cardarelli continue to overlook key text in documents, including examples that I do not deal with until Part 2. Table 2 at the end of this introduction presents a full list, discussed item by item in the introduction or in my annotations of their published rebuttals. I now reply to the issues raised in the rebuttals that I seem key.

Key disputes that arose in the rebuttals.

1) Failure to rebut Calabrese's overlooking of key historical text about uncertainty in the 1956 NAS genetics report

Calabrese made no attempt to rebut his overlooking of language in the 1956 genetics report that acknowledged the great uncertainty in the illustrative calculations of total mutants -- acknowledgements that voiced the concerns of those who had written earlier about them to chairman Weaver and raised them at meetings. The caveats included:

"… clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known."

"… some geneticists find this concept difficult and illusive."

"… different geneticists find meaningful rather different approaches to the problem of genetic damage."

"… they recognize that there is a risk involved in extrapolating from mouse and Drosophila data to the human case."

"… this kind of estimate is not a meaningful one to certain geneticists…"

"… This calculation is admittedly somewhat complicated and disappointingly vague.

"… It is, to some geneticists, not a very meaningful way of looking at the problem."

Table 1. Seven qualitative statements from the 1956 NAS genetics report[a] about uncertainty in damage calculations, which reveal for the reader the differences between, and concerns among, committee members about such calculations – statements ignored by Calabrese.

Statements from the final NAS report.	**Committee members whose concerns were covered by the statements, not suppressed**.[b]
"There is a considerable margin of uncertainty about much of this, and as a result, there are naturally some differences of opinion among geneticists them-selves as to exact numerical values, although no disagreement as to fundamental conclusions..." at p. 1158.	An indication of differences among the committee, although not as to fundamentals. No disagreement about the LNT response.
"Measuring total damage in terms of the number of mutations does indeed necessarily involve this concept of the average damage from a typical mutation, and some geneticists find this concept difficult and illusive." At p. 1160.	An indication of division on the committee.
"Rather than utilizing this concept of the average total damage per mutation, some geneticists prefer to start with a consideration of the tangible damage which occurs now, as a result of the current rate of mutation…" At p. 1160.	Covers concerns of Sewall Wright about total mutation calculations.
"This calculation is admittedly somewhat complicated and disappointingly vague. It is, to some geneticists, not a very meaningful way of looking at the problem. To others it adds up to something at least reasonably clear, and in any event very serious." At p. 1163.	An indication of division on the committee and an expression of inherent limitations of the calculations.
"Second, this kind of estimate is not a meaningful one to certain geneticists…it is much harder still, at the present state of knowledge, to translate this over into a recognizable statement of harm to individual persons. Also they recognize that there is a risk involved in extrapolating from mouse and Drosophila data to the human case." At p. 1163.	Covers the written concerns of James Neel and Sewall Wright, and possibly Demerec. Inidicates that the committee was divided.
"And this is the point at which it becomes most clearly evident that different geneticists find meaningful rather different approaches to the problem of genetic damage." At p. 1163.	An indication of the division in the committee.
"Concerning these estimates of total number of mutants, three things should be said. First, they are clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known. " At P. 1163.	This is a catch-all disclaimer for all the geneticists on the committee. No geneticist on the committee found the calculations "really satisfactory." The second sentence covers particular concerns of Sturtevant and Wright.

a) (Weaver et al. 1956), which is the version reprinted in *Science*, to which page numbers in the Table refer. The original and complete report can be found here: (NAS 1956a). The quotations in the Table also appear in footnote 23.

b) Calabrese in rebuttal points to critical correspondence written to chairman Weaver prior to the final report, overlooking how Weaver handled these disagreements in the final text by embracing the uncertainty and limitations in the damage calculations.

Calabrese in rebuttal points to critical correspondence written to chairman Weaver prior to the final report, overlooking how Weaver handled these disagreements in the final text by embracing the uncertainty and limitations in the damage calculations. Table 1 lists the seven qualitative disclaimers placed in the 1956 final report and matches them to various committee members who had complained about or disagreed with various aspects of the total mutant calculations presented in the report.

The most that the report could conclude was that the results of the calculations were very sobering and to some geneticists very serious. It is not surprising, then, that these calculations were not used for any recommendation of the committee. Their recommended allowable dose to the public was based on not exceeding 10 R in the first 30 years of life, which amounts to no more than a doubling of yearly background dose rate.

There is no basis here for any charge by Calabrese that the committee covered up uncertainty, which was Calabrese's major charge of scientific misconduct. He overlooked the disclosures written in the final document, which had been written to allow committee members to sign off on it --- standard NAS practice with non-consensus material.

2) Who among us uses primary documents and who uses them correctly?

I have been told that the primary-document defense used by C. and C. in their rebuttals has gained some traction among non-historian audiences. Their rebuttals use the language of primary and secondary documents but not in the way that historians use them, as reflected for instance in the statement on professional standards of the American Historical Association (AHA 2023) at Section 2. As a result of this misuse, Calabrese wrongly claims that I use secondary documents exclusively, and thus I do not use primary

documents. Even summaries of my work generated by AI search engines now include statements like, "some say he does not use primary documents," which raises unfounded doubts about my work. It also diverts attention from my allegations that Calabrese interprets primary documents incorrectly.[11] I have found that he ignores chronology, picking out statements in correspondence or transcripts about uncertainty or disagreements made prior to the finalization of reports, failing to check the wording of the final reports to see how the uncertainty was actually handled and how the different points of view of committee members were presented to get them all to sign off on the reports.

How does Calabrese justify claims that I do not use primary documents? First, he tries to imply that documents from the time period of interest on which I rely to contradict him, which include final reports, do not constitute primary documents because they are in the public domain. He says:

> "Beyea's analysis is entirely based on what is present <u>in the public domain</u> within the realm of journal publications, dissertations, NAS activities/reports, and government reports."

[11]" He does not, I argue, follow the professional practice of history as he claims he does. The AHA has a code of conduct, which requires fair mindedness: "...contributing in a fair-minded way to ongoing scholarly and public debates over what those sources tell us about the past and also what they fail to illuminate."

Also, "...historians also understand that all knowledge is situated in time and place, that all interpretations express a point of view, and that no mortal mind can ever aspire to omniscience. Because the record of the past is so fragmentary, absolute historical knowledge is denied us."

And "They believe in vigorous debate, but they also believe in civility." A belief that Calabrese fails to follow in his attacks on the work of Jolly.

AHA. Statement on standards of professional conduct (Available at: https://www.historians.org/resource/statement-on-standards-of-professional-conduct/ Accessed 25 January 2026) [online]. Available at: https://www.historians.org/resource/statement-on-standards-of-professional-conduct/. At Section 2.

The ease of availability has nothing to do with primary vs. secondary. The AHA defines the terms as follows:

> "By 'documents,"'historians typically mean all forms of evidence— not just written texts, but artifacts, images, video, statistics, oral recollections, the built and natural environment, and many other things—**that have survived as records of former times.** By "secondary literature," we typically mean all subsequent interpretations of those former times based on the evidence contained in primary documents." (AHA 2023) at Section 2. Bold formatting added.

The documents that I use to contradict Calabrese's interpretations have "survived as records of former times." They are primary documents. Calabrese has his own personal definition, however, which does not match that of historians. He implies that I rely on "The exclusive use of secondary sources …", that I live "fulltime in the world of secondary sources.' Thus, he thinks that "journal publications, dissertations, NAS activities/reports, and government reports" are all secondary documents. Readers should bear this in mind when considering his criticisms.

To the above partial list of my sources stated by Calabrese, I would especially add transcripts of meetings, which I discussed repeatedly in my 2017 paper (Beyea 2017). Calabrese wants to claim these for himself and not reveal that I claim he has interpreted them incorrectly. I use all of these primary documents to show that Calabrese overlooks key historical text leading to false premises about those documents. Readers can decide if Calabrese should have directly answered my allegations based on them.

To justify his claim about my reliance on secondary documents, Calabrese makes one of his standard mistakes, omitting contradictory language from the same sentence from which he quotes. This time he takes language out of context from my 2017 paper in an attempt to imply that I relied *solely* on secondary documents for a key evaluation. I had stated:

> "Based **solely on secondary sources written by historians**, I have not been able to figure out exactly which committee members' calculations went into generating the 50,000 tangible mutation estimate, but it is clear that Demerec's low estimate must have been one of them."

Calabrese, however, omits key words in the sentence.

> "For example, Beyea (2017) tried to characterize mutation risk estimates of the BEAR I Genetics Panel (1956) **based, as he writes, "solely on secondary sources by historians,**" leading him to offer unverified speculative estimates when the primary documents were available." (Bold formatting added.)

This is completely misleading and untrue. He omits key parts of the sentence, e.g., the conditional word, "Based," just as he did with Muller, deleting the words, "we believe," which changed the meaning to allow Calabrese to call Muller a liar (Beyea 2025 at p. 516). Such a desperate ploy of distorting my words to justify his claim that I relied exclusively on secondary sources is yet again evidence that he has no real rebuttal to the false premise charges, only attempts to mislead and distract.[12] Unfortunately, in the absence of an opportunity for me to push back in the journal, Calabrese's framing provides powerful rhetoric, repeated by Cardarelli, which will likely convince his supporters and those of Cardarelli that they need not worry about my allegations. They both ignored my discussion of the multitude of sources on which I relied in both my 2017 and 2025 critiques. They both proceeded to set up a straw man with which to distract the reader from my allegations that Calabrese missed key historical text (in primary documents).

Now, I do use some secondary documents written by historians, although in limited ways, as has Calabrese. I have previously relied in one case on the many historians who

[12] With lapses like these, it is no wonder that a NAS committee would not cite him on radiation risk or radiation history (citation to Cardarelli). Put this in the line-by-line,maybe.

have written about this period based on their readings of correspondence. For example, in my 2025 paper, I cited Beatty for a statement about Sewall Wright:

> "Calabrese is apparently unaware that one of the excluded three committee members (Sewall Wright) did not want his estimate used (Beatty 2006),

Beatty's assessment of his sources is disputed in the rebuttals, so for this response I go directly to Wright's letters to Warren Weaver, the chairman of the 1956 committee. Although not stated explicitly in my 2025 paper, I also relied on Beatty and Jolly to characterize Demerec's type of damage estimate, which Calabrese in a recent article has confirmed (Calabrese and Selby 2026) at Table 5, so there is no dispute. I have also made use in part of autobiographies, which fall somewhere in between primary and secondary categorization.

Calabrese cannot use the label of primary source as a shield against criticism and an excuse for not directly responding to the criticisms I have made about his oversights in primary documents, such as key passages or sentences in the report of the 1956 NAS genetics committee or in papers of Uphoff and Stern. If Calabrese is to rely on primary documents, he should use them correctly. He does not. I have shown that he is prone to selective stakeholder perception, which leads him to omit evidence from documents that run counter to his interpretation of the history and to overlook documents that run counter to his interpretation. Given his private-correspondence defense, I have here examined correspondence to show that even here he overlooks key text from correspondence that runs counter to his interpretation, particularly with respect to his claim that historian Beatty was wrong about Sewall Wright's damage estimate and his implication that Ernst

Caspari was in opposition to the work of Delta Uphoff.[13] Correspondence is a complex network of chronological sources that can mislead if only tidbits are extracted and not checked for contradictions. I note that Calabrese also relies heavily on secondary sources, including his previous writings about the early period and the work of historians, like Jolly, and he relies on hearsay in correspondence.[14]

3) *Failure to concede that A-bomb genetic effects were reported in the 1956 NAS genetics report*.

An example of misreading of text by Calabrese and Cardarelli is the omission of the report's mention of quantitative values for the genetic effects measured in Japan in the early years after the detonation of the A-bombs, which contradicts the claim that the report did not take into account early genetic data from Japan. The NAS genetics report presents a value for the doubling dose lower limit obtained from Jame Neel's data, as discussed below.

Cardarelli claims he checked Calabrese sources, at least those shown to him. I wonder in connection with the claim that the 1956 NAS Committee did not take into

[13] Calabrese has repeatedly implied without evidence that Caspari kept quiet out of career self-interest. Calabrese EJ. Societal threats from ideologically driven science. Academic Questions; 2017c., Calabrese EJ. Ethical failings: The problematic history of cancer risk assessment. Environ Res 193: 110582; 2021., Calabrese EJ. Thresholds for radiation induced mutation? The Muller-Evans debate: A turning point for cancer risk assessment. Chemico-Biological Interactions 382: 110614; 2023., Calabrese EJ. Cancer risk assessment, its wretched history and what it means for public health. Journal of Occupational and Environmental Hygiene 21: 220–238; 2024..

[14] An example of his use of secondary sources is his frequently used quote about Demerec wanting to "save the hit model." This is not from Demerec directly but from the recounting by Caspari in a letter to Stern. Yet, one cannot guess this from Calabrese's writing, unless one checks out the citation. We don't know exactly what Demerec said, so the true source should have been mentioned in the text. Making matters worse is that Calabrese claims that Demerec said things that do not appear in the Caspari letter. Calabrese claims that Demerec said, "What can we do to save the hit theory" or "What can be Done to Save the Hit Model?"

account the A-bomb results from James Neel, was Cardarelli shown the 22 February 1956 letter from NAS committee member Sewall Wright to chairman Warren Weaver in which Wright shows sophisticated knowledge of Neel's A-bomb results for use in doubling dose calculations?[15]

> "At the other extreme, the absence of significant effects on percentages of stillbirths and neonatal deaths, on gross abnormalities, on weight at birth and on measurements at 9 months and only doubtfully significant effects on sex ratio in Dr. Neel's analysis of 3000 children of Japanese parents, at least one of whom had been exposed before conception to 200-600r makes it fairly certain that the doubling dose is considerably more than 3r." From a 22-February 1956 letter from Neel to Weaver (Weaver 1956b).

Was Cardarelli shown the actual text in the 1956 NAS Genetics Committee report that mentions the Japanese lower limit doubling dose?

> "The experience in Japan gives some basis for thinking that the doubling dose is larger than 5 roentgens." (Weaver et al. 1956 at p. 1162).

Was Cardarelli shown the 23 January 1956 letter from Neel himself to Weaver? It states,

> "I do not doubt that radiation at all levels produces mutations." (Weaver 1956a).

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[15] The reference in the quotation to 200-600r connects the data Wright is using to the 1956 report of Neel and Schull (Section 4.8) rather than the 1953 report, which used 200-300r for Category 5 exposure. However, I have not been able to locate in the 1956 Neel report the data for the 3000 parents. Table 2.1 gives about 3700 parents in Categories 4 and 5, at least one of whom had been exposed before conception to level 4 or 5 doses. I found no figures listed separately for Category 5. I presume Wright got his data directly from Neel. However, it is possible that the data comes from an earlier presentation by Neel, after 1953 but before the 1956 paper. It is also possible that others on the committee made similar calculations that contributed to the final 5 r lower limit on doubling dose from Japan that appeared in the report.

The Report states: "The experience in Japan gives some basis for thinking that the doubling dose is larger than 5 roentgens." "Larger than 5r may reflect Weavers translation to "larger" from Wright's "considerably more than 3r." The statement about 5r is distinct from the lower limit the committee assigns to the doubling dose, because there is another way to get to it, namely through assessment of background radiation. On March 22, Wright writes to Weaver, "As to the minimum, this is clearly 5r (not 3r) if 5r (bottom of p. 13) is accepted as the total dose per generation from natural causes. At p. 4 of Weaver W. Correspondence 1956 3-16 to 3-27 from Muller mss files at Lilly Library. 1956c.

Had Cardarelli been shown such primary evidence, I don't see how he could have concluded that the 1956 NAS Genetics Committee did not take into account Neel's data.

Moreover, Neel's reluctance to make quantitative estimates of human mutation risk from ionizing radiation was noted in the report:

> ""Second, this kind of estimate is not a meaningful one to certain geneticists…it is much harder still, at the present state of knowledge, to translate this over into a recognizable statement of harm to individual persons. Also they recognize that **there is a risk involved in extrapolating from mouse and Drosophila data to the human case.**" (Weaver et al. 1956 at p. 1163). Bold formatting added.

The dispute between Neel and Muller, carried on during and after the 1956 report preparation, was not about dose threshold for mutations, as might be assumed by some readers from Cardarelli's discussion of it (Cardarelli et al. 2023 at p. 134). As the last chapter of Neel's 1956 report with Schull makes clear (Neel and Schull 1956), the dispute was about the difficulty and desirability of making quantitative genetic risk estimates in 1956, particularly when it came to using animal data. That last chapter (15), not previously part of the published results about A-bomb genetic risks, was circulated to the Committee. The bulk of the geneticists on the 1956 committee disagreed with Neel's negative views on calculating risks, but his concerns and those who agreed with him to some degree (e.g., Sewall Wright) were presented in the report as indicated in the above quotation. See footnote 23 for additional caveats included in the report about quantitative calculations that incorporate concerns expressed to chairman Weaver by various members of the committee.

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4) Was the video series only about history?

Consider this excerpt from Cardarelli's response to my Part 1:

> "Again, Beyea missed the point of the HPS video series (and associated publication). It was to present the history of the LNT model, not to validate or refute its use in cancer risk assessment." (Cardarelli 2026) p 19 # 63

Calabrese claims

> "If they had done so, they would have known that Beyea's allegation that I argued for the elimination of LNT was false based on watching episode 22." (Calabrese 2026 at p. 495).

We certainly saw a different video series. I don't obviously know what Cardarelli saw or recalls, or what Calabrese recalls, but the raw transcript of episode 22 lays out Calabrese's views on getting rid of the LNT:

> "But we need to have some movement from this and to continue to rely upon a linear dose response when it has been based upon studies that have been shown to be to be flawed, to be fraudulent, to be loaded with bias, to have your Your behaviors you are in we have inherited a history. It's a distasteful history by leaders in our field that have brainwashed us to believing their flawed science and their ideological science. And I don't really want to follow a science that has been based upon ideological sciences that have been in the workings of all the bias of like Lewis and Mueller and others, it's it's corrupted our regulatory practice." Episode 22, before timestamp 24:16.

In this episode Calabrese also discusses his desire for a "compromise" dose response model that is zero or beneficial below a threshold point determined by what he calls optimization (discussed in a paper he published in HPJ (Calabrese et al. 2016)[16]). His proposal "… could, if needed, readily default to a threshold model." (Calabrese et al. 2016). Calabrese is acting as an anti-LNT activist, not a history professor. In Episode 22 he discusses his low risk, minimum dose proposal that he offers as an alternative to reliance on the no threshold part of the LNT below a risk of 1 in 10,000:

[16] "The optimal dose is at the nadir of the hormetic dose–response, which occurs at approximately the dose corresponding to 10−4 risk using the LNT model (Calabrese et al. 2015). This regulatory sweet spot provides the lowest tumor incidence based on an integration of the risk estimates of both models." And, later: "Though the hormetic model is complex, it could, if needed, readily default to a threshold model with a long history of use in the risk assessment community and federal regulations."

> "Now, if, if I'm wrong, totally wrong, and the LNT is is correct, then the amount of adverse effects in the population. I mean, it would obviously incur, it would increase but but it would still be relatively modest to be one in 10,000 compared to a normal expectation of one in three. So it's still a modest risk."

He is OK with the LNT above his minimum risk threshold, but he clearly called for the elimination of no threshold, which is the key part of the LNT, arguing that the risk would be small for everybody if he were to be wrong about hormesis/threshold

.

This statement was made in response to a question by interviewer Barbara Hamrick about how to revise the current framework. Calabrese, not being a health physicist, did not put this in terms of a minimum dose of concern. I do so here. The standard A-bomb cancer relative risk number is 0.5 per Sv averaged over both sexes (Grant et al. 2017) at Table 5. Therefore, a 1-in-10,000 relative risk would occur under the linear model at 2 ten-thousandths of a Sv, or 1/5th of a mSv, or 20 mrem.[17] This proposal for a cutoff of concern is modest compared to those being proposed today, including the proposal put forth by the HPS authors in their review of the video series, which is 100 mSv, or 10,000 mrem. In fact, Calabrese's acceptable risk number is 500 times more protective than Cardarelli's.

Cardarelli claimed in his rebuttal that, "Our paper did not call for changes to regulatory policy." Yet, the bullets in his article that he quotes in support clearly do call for such a change based on the actual language:

> "A. Re-examine the current regulatory and cancer risk models by **replacing the most fundamental assumption** (that any increase in dose is an increase in risk) with a true null hypothesis (that there is no effect)." P. 7, Item 19. Bold formatting added.

[17] Calabrese does not specify if this is a single or cumulative exposure,

That replacement would certainly eliminate no threshold from regulation and cancer risk models. It would mean a threshold up to the point where effects could be proven (according to an unspecified standard), as is made clear in Bullet D below. The HPS authors want to:

> "D. Harmonize radiation protection by considering all dose response models and **applying the LNT model only to the point where adverse health effects are observed (e.g., above 100 mSv or 50 mSv y-1**)." P. 7, Item 19. Bold formatting added.

This is another clear call for replacement of no threshold. All of this negates the idea that the video series was "to present the history of the LNT model, not to validate or refute its use in cancer risk assessment." The many claims by Calabrese in the series that the no-threshold theory was fraudulent and dishonest, based on falsification, fabrication, and corrupted data, sometimes repeated by Cardarelli at the end of episodes, leads me to believe that one of the motives behind the series was to convince viewers that the LNT was fraudulent. This was not an unbiased history of the LNT model that an academic might present.[18] Cardarelli as the producer gave an exclusive forum to a person whose negative views of the LNT and claims of scientific misconduct have been known for a decade. Cardarelli knew what he was getting, and he ignored critical views of Calabrese's positions, such as mine. These videos will be on the web for a very long time, tying the HPS to Calabrese's accusations. Cardarelli claims that the videos state that the views expressed do not represent the views of the HPS, but I did not find such a disclaimer in his ending recaps, but only on the bottom of the final screenshot after the final credits, which few will see.

[18] For example, Episode 13 of the video series is entirely devoted to criticism of the LNT, the philosophy behind its use, its ALARA implementation.

5) *The video series was a form of investigative journalism, not science, with but one source*

The video series was a form of investigative journalism, with Cardarelli as the host narrator. He only has one source (Calabrese) to rely on, not the two sources that are commonly required in journalism for verification. Instead, Cardarelli claims that he required Calabrese to show him documentary evidence. This brings up memories of the proverbial ham sandwich, as in the saying that prosecutors can get a grand jury to indict a ham sandwich because of their control over the evidence shown and its interpretation. Calabrese could get Cardarelli to condemn early researchers supporting no threshold I argue, because Calabrese was the one showing the evidence and interpreting it. In addition, given Cardarelli‘s past views against the LNT model (Cardarelli and Ulsh 2018), it may not have been a difficult task to convince him

In light of Cardarelli’s summaries at the end of video episodes, where he repeats as truth Calabrese’s derogatory claims about early researchers, it is clear that he is completely on board. A problem with having only one side showing you evidence is that it may not be complete, especially when the evidence comes from opposition research to a competing theory of dose response. There is an enormous amount of correspondence, reports, and journal articles to check and Cardarelli couldn’t check them all.

Cardarelli could have asked me to comment on Calabrese’s evidence. He knew about my alternate views because in 2018 he noted that my 2017 paper was in opposition to Calabrese (Cardarelli and Ulsh 2018). He also lists two of my relevant publications at

the top of a rolling list of papers shown at the end of Episode 11.[19] Had he contacted me during the creation of the video series as a check on Calabrese's evidence, he would not have been limited to Calabrese's selections alone. Alternatively, he could have used my 2017 paper (Beyea 2017) as a basis for discussion with Calabrese about many of the documents shown to Cardarelli. At a minimum, to inform viewers, he should have said that Calabrese's views on the 1956 NAS genetics committee and on the Stern group are contested.

6) *"Six vs nine geneticists;" Unmentioned correspondence of Sewall Wright supports historian Beatty, not toxicologist Calabrese.*

Consider another example of Cardarelli being shown selective evidence, namely evidence about the "six vs nine geneticists," a topic about which Calabrese has expended a great deal of ink over the years. The question at issue involves how many geneticists made illustrative calculations of genetic damage given in the report of the 1956 NAS genetics committee, an issue by the way that has nothing to do with threshold or linearity.

The report states,

> "Six of the geneticists of this committee considered the following problem: suppose the whole population of the United States received one dose of 10 roentgens of radiation to the gonads. What is the estimate of the **total number of mutants** which would be induced by this radiation dose and passed on to the next total generation of about 200 million children? Each geneticist calculated what he considered to be the most probable estimate, **and then bracketed this by his minimum and maximum estimates**." (Weaver et al. 1956 at p. 1163). (Bold formatting added.)

[19] Episode 11 does not reflect any of my critiques of Calabrese's work, as far as I can tell, so I don't know why my papers were put on the list.

Calabrese claims there were really nine estimates submitted, all of whom should have been counted in the average and computed range. This is a key part of Calabrese's claim about scientific misconduct on the part of the 1956 Committee and a key part of my 2017 refutation, to which Calabrese never responded.[20] He attempts to respond in part to my 2017 paper at last in his current rebuttal, but once again fails to confront the estimate-defining language in the 1956 report quoted above.[21] Note the restriction in the above quote to "total mutants" and the requirement to bracket by minimum and maximum estimates. Estimates not meeting that category would not and should not have been included.

Had I been asked to comment on Calabrese's quotes about this from the 1956 NAS genetics committee report, I would have pointed out that the six geneticists estimated totals mutants only and gave a range around their most probable values. Those estimates that did not give total mutations, but only tangible (visible) mutations and not recessives, belonged in another category. Tangible estimates were given in a different place in the NAS report. Tangibles were defined as imminent dangers:

[20] He now says at page 5 that it was pointless for him to respond: "In fact, these failings made communicating with him via letter to the editor nearly pointless." In addition, I point out, it would have given me an opportunity to reply to such a letter. However, he could have written a separate article in response that would not have been a letter to the editor. He did not. To the contrary, shortly after my paper appeared, he published a reiteration of his narrative in the journal he edits, *Dose-Response*, with no changes related to any of my criticisms. I was disappointed. His paper was accepted 4 days after submission, according to the published statistics Calabrese EJ. The Mistaken Birth and Adoption of LNT: An Abridged Version. Dose-Response 15: 1559325817735478; 2017b.

[21] He does muddy the waters by saying about the quotation, "That is, the estimate is presented for a single (i.e., the next) generation, not the 10 generations that Beyea claims or possibly Total Damage that affects all descendants." Total damage includes recessives not all of which are tangible in the first generation and get passed on to future generations. There are two categories discussed in the 1956 report, tangible and total. These categories separate the six estimates from at least two others made by geneticists. The third may have been excluded for lack of bounding estimates. Calabrese will not face this fatal flaw in his argument.

> "A discussion of genetic damage necessarily involves, on the one hand, certain tangible and imminent dangers, certain tragedies which might occur to our own children or grandchildren; and on the other hand certain more remote trouble that may be experienced by very large numbers of persons in the far distant future." (Weaver et al. 1956 at p. 1159).

The report gave a quantitative estimate of these tangible mutations after a dose of 10 roentgens to the gonads:

> "A dose of 10 roentgen would, on the above basis, give rise to some 50,000 new instances of tangible inherited defects in the first generation,…" (Weaver et al. 1956 at p. 1163).

The word, tangible, does not appear in any of Calabrese's papers in connection with this context, based on my digital search of his papers, indicating that he never discussed or quoted the above part of the 1956 NAS report. Had Cardarelli asked me to comment on the 6-vs.-9 issue, Calabrese would have finally been forced to address this contradiction first pointed out by me in 2017 (Beyea 2017). Calabrese would have had to prove to Cardarelli during the preparation of the video series that his claimed missing three geneticists actually estimated total mutations and not a lesser category and that they also provided minimum and maximum estimates. Since that does not appear to be the case, as I discuss below and in part in 2017, it is likely that such scrutiny would have ended up sparing viewers of the video series from misleading information and eliminated a long section of Calabrese's 44 page rebuttal.

To his credit, Cardarelli, unlike Calabrese, actually discussed the sentence in the NAS 1956 genetics committee report that gave a rate for estimated tangible mutations per dose:

> "There is a discussion of tangible mutations, but nowhere does it state that it results from two panel members' calculations; therefore, I was unable to validate Beyea's statement." (Cardarelli 2026) at the 12th page.

He has things backwards here. The accused does not need to prove innocence. It is the accuser that needs to prove guilt. It is up to Calabrese to show that any missing estimates were for total mutations and not tangible ones. It would violate the conditions of the sentence in the report to include estimates that were not for total mutations. Logically, we would expect estimates of tangible (or visible) mutations to appear in the averaging of tangible emissions and not to be mixed in with the averaging of total emissions. Calabrese has not offered any explanation, as far as I know, as to why estimates of tangible emissions should have been added to total emissions for the final report. True, it would have increased the range of estimates as Calabrese states, but it would have been an inappropriate action, given the stated criteria for the total-damage estimates. By separating estimates of different types, the committee made the correct scientific choice.

Although Calabrese has the burden, I will offer evidence that both Demerec and Wright did not calculate total mutations, and that Moser did not submit minimum and maximum estimates, although including his estimate would have made little difference. Calabrese and Selby in a recent publication have indicated that Demerec calculated less than total mutations:

> "He noted that the mutations were all visible mutations and that ratio of visible to lethal mutations was needed to get an estimate for all mutations. He suggested such data would exist for Drosophila. Then he stated that it would be necessary to know what fraction of the total mutations were detrimental." (Calabrese and Selby 2026) at Table 5.

As for Wright, consider this claim by Cardarelli as presented in his rebuttal:

> "Beatty 2006 states (p. 63) that "In the end, Wright refused to be party to this particular activity." Episode 12 (timestamp 15:10) shows the cover page of nine reports submitted by the panel members, including Wright, who took up the challenge. Beatty was wrong, and therefore Beyea was wrong. Calabrese was right." (Cardarelli 2026) at 12th page.

But what type of calculation did Wright make? Did he estimate total mutations? No, he did not. He explicitly stated in a letter to Chairman Weaver dated 22 March 1956 that he did not believe that calculating total mutations was the correct way to estimate hereditary damage.

> "I fully agree with Jim Neel's statement that it is hard enough to calculate the number of mutations in a given population, but almost infinitely harder "to interpret the biological impact of these mutations on the population". I still think that the most reliable and intelligible index of genetic damage from the personal viewpoint is that based on **tangible genetic defect**, the estimate of the doubling rate, and estimated increase in the next few generations (**largely excluding recessive and dominants of low penetrance**)." (Weaver 1956c at p. 4). (Bold formatting added.)

Wright also exclusively weighted mutations based on their burdensomeness to the families.[22] In a late, May 22nd letter to Weaver, Wright repeated his reasons for his "failure to be impressed by the estimates referred to." He also aligned himself with the category in the report of those to whom "… this kind of exercise is not a meaningful one …", giving his reasons. (Weaver 1956e at p. 4).

Thus, Wright did not go along with the exercise that the other six did. Beatty has correctly stated, "Again, in the end, Wright refused to be party to this particular activity." (Beatty 2006 at p. 63), this particular activity being estimation of total mutants.

A lesson here is that I was correct in 2017 to rely on a secondary source from a historian of the period, Beatty, and not to accept Calabrese's reasoning from a subset of

[22] "It seems to me that any serious attempt to look at radiation damage from a personal viewpoint must give different kinds of mutation enormously different weights according to the personal distress and burdensomeness to the families that they cause, My own calculations were made exclusively from this viewpoint." Weaver W. Correspondence 1956 3-16 to 3-27 from Muller mss files at Lilly Library. 1956c..

correspondence. The principle espoused by Cardarelli and Calabrese that Calabrese's primary-sourced information trumps information in secondary sources from historians is shown to be false in this key case.

Wright's estimate should not have been included with the total mutation estimates of the six geneticists to be consistent with the descriptive language of their task. And I'm not sure it should have been included with the average for tangible mutations either, because of the weighting of mutations by burdensomeness, which is not part of the descriptive language for the "tangible" estimate discussed in the report.

In an NAS report, it is misleading to take correspondence or transcripts as an indicator of the final decisions made in a report. Discussions and compromises can be worked out at breaks and report writing sessions. Committee leaders are often negotiating language up to the last minute to get approval of all members, as I know from experience. In this case, there was a 12-13 May meeting at which changes were agreed to by the participants attending as stated in a 15 May letter to the Committee from the chairman, Warren Weaver (Weaver 1956d) although I have not seen a transcript of that meeting of a subset of the committee.

There were numerous telegrams sent or phone calls at the end of the process (Weaver 1956e 29 May 1956 at pp. 1, 2). The report language itself is the final product that members sign off on, not letters or statements of individual members.

The last member who submitted a number for mutations after a 10 Roentgen exposure was Kaufmann, who passed on a calculation by Moser, which was similar to other calculations as to most probable value, as confirmed by Calabrese and Selby (Calabrese and Selby 2026). However, Moser did not submit minimum and maximum

values, also confirmed by (Calabrese and Selby 2026), which was a requirement for the average presented in the 1956 report, which stated:

> "Each geneticist calculated what he considered to be the most probable estimate, and then bracketed this by his minimum and maximum estimates." (Weaver et al. 1956 at p. 1163).

In any case, had the Moser total estimate been included, it would not have changed the most probable value by much according to the Table presented by (Calabrese and Selby 2026).

Thus, the decision to exclude three estimates from total mutants was a correct decision given the criteria chosen. Calabrese may have wanted different criteria used and wanted the report written differently with different bundling of the estimates and uncertainty, but he was not on the Committee. He has his own axe to grind as an advocate for a competing theory of dose response.

Table 1. Seven qualitative statements from the 1956 NAS genetics report[a)] about uncertainty in damage calculations, which reveal for the reader the differences between, and concerns among, committee members about such calculations – statements ignored by Calabrese.	
Statements from the final NAS report.	**Committee members whose concerns were covered by the statements, not suppressed**.[b)]
"There is a considerable margin of uncertainty about much of this, and as a result, there are naturally some differences of opinion among geneticists them-selves as to exact numerical values, although no disagreement as to fundamental conclusions..." at p. 1158.	An indication of differences among the committee, although not as to fundamentals. No disagreement about the LNT response.
"Measuring total damage in terms of the number of mutations does indeed necessarily involve this concept of the average damage from a typical mutation, and some geneticists find this concept difficult and illusive." At p. 1160.	An indication of division on the committee.
"Rather than utilizing this concept of the average total damage per mutation, some geneticists prefer to start with a consideration of the tangible damage which occurs now, as a result of the current rate of mutation…" At p. 1160.	Covers concerns of Sewall Wright about total mutation calculations.
"This calculation is admittedly somewhat complicated and disappointingly vague. It is, to some geneticists, not a very meaningful way of looking at the problem. To others it adds up to something at least reasonably clear, and in any event very serious." At p. 1163.	An indication of division on the committee and an expression of inherent limitations of the calculations.
"Second, this kind of estimate is not a meaningful one to certain geneticists…it is much harder still, at the present state of knowledge, to translate this over into a recognizable statement of harm to individual persons. Also they recognize that there is a risk involved in extrapolating from mouse and Drosophila data to the human case." At p. 1163.	Covers the written concerns of James Neel and Sewall Wright, and possibly Demerec. Inidicates that the committee was divided.
"And this is the point at which it becomes most clearly evident that different geneticists find meaningful rather different approaches to the problem of genetic damage." At p. 1163.	An indication of the division in the committee.
"Concerning these estimates of total number of mutants, three things should be said. First, they are clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known. " At P. 1163.	This is a catch-all disclaimer for all the geneticists on the committee. No geneticist on the committee found the calculations "really satisfactory." The second sentence covers particular concerns of Sturtevant and Wright.
c) (Weaver et al. 1956), which is the version reprinted in *Science*, to which page numbers in the Table refer. The original and complete report can be found here: (NAS 1956a). The quotations in the Table also appear in footnote 23. d) Calabrese in rebuttal points to critical correspondence written to chairman Weaver prior to the final report, overlooking how Weaver handled these disagreements in the final text by embracing the uncertainty and limitations in the damage calculations.	

In addition, Calabrese and Cardarelli always ignore the uncertainty caveats the report gives for illustrative risk calculations and risk in general,[23] which contradicts the claim that the report understated uncertainty for these calculations.[24] As I stated in 2025:

> "If one reads the report section where the calculations are called disturbingly vague and not meaningful to some geneticists, it is hard to see how anyone could consider that the 1956 report hid uncertainty." (Beyea 2025 at p. 514).

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[23] "There is a considerable margin of uncertainty about much of this, and as a result, there are naturally some differences of opinion among geneticists them-selves as to exact numerical values, although no disagreement as to fundamental conclusions...." Weaver et al.. 1956 at p. 1158.

"Measuring total damage in terms of the number of mutations does indeed necessarily involve this concept of the average damage from a typical mutation, and some geneticists find this concept difficult and illusive." At p. 1160.

"Rather than utilizing this concept of the average total damage per mutation, some geneticists prefer to start with a consideration of the tangible damage which occurs now, as a result of the current rate of mutation..." At p. 1160.

"And this is the point at which it becomes most clearly evident that different geneticists find meaningful rather different approaches to the problem of genetic damage." At p. 1163.

"Concerning these estimates of total number of mutants, three things should be said. First, they are clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known. " At P. 1163.

"Second, this kind of estimate is not a meaningful one to certain geneticists...it is much harder still, at the present state of knowledge, to translate this over into a recognizable statement of harm to individual persons. Also they recognize that there is a risk involved in extrapolating from mouse and Drosophila data to the human case." At p. 1163.

"This calculation is admittedly somewhat complicated and disappointingly vague. It is, to some geneticists, not a very meaningful way of looking at the problem. To others it adds up to something at least reasonably clear, and in any event very serious." At p. 1163.

[24] For the record, I want to indicate that the factor of 10 given for the uncertainty in calculations was specifically for the **total mutant** calculations, and it applied to how an individual estimate fluctuated. Here is the language in the report: "These six geneticists concluded, moreover, that the uncertainty in their estimation of the most probable value was about a factor of 10. That is to say, their minimum estimates were about 1/10, and their maximum estimates about 10 times the most probable estimate. " at p 1163 of Weaver et al., 1956. The actual individual estimates fluctuated above and below a factor of 10, but the geometric mean of the factors I calculated to be about 15. I do not know for sure how the committee actually did their factor-of-10 calculation, but I see my factor of 15 as equivalent for an NAS report. I discuss the many uses of the geometric treatment of large numbers by committee members in my annotation of Cardarelli at item 40.

The report also states:

> "Concerning these estimates of total number of mutants, three things should be said. First, they are clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known. Second, this kind of estimate is not a meaningful one to certain geneticists." (Weaver et al. 1956 at p. 1163).

How could anyone reading this quotation and the others in Table 1 maintain that the report hid the uncertainty in the illustrative calculations? The total uncertainty was not minimized.

And what impact did this particular exercise have on anybody's thinking? Calabrese has no evidence that exclusion of the section on total mutations would have made any difference as to the impact of the report. The section on tangible mutations would also seem to me to have been scary enough, along with the idea that there was no threshold for genetic damage. The 1956 genetics report does not mention cancer. Finally, the values of the estimates, made for illustrative purposes, do not have anything to do with acceptance of the linear no-threshold model. They have to do with the seriousness of the effects of a particular dose. Any overestimate is relevant to the impact the committee may have had on fear of radiation, although I doubt a particular number made any difference. In any case, Calabrese and Cardarelli should have stuck to their idea of overestimate, which Cardarelli brings up repeatedly in his rebuttal, despite its irrelevance to LNT as a dose response.

7) Historian Jolly's analysis was not flawed as Calabrese claims

In his rebuttal, Calabrese also takes on historian, Jolly, for his handling of the geneticists making risk calculations for the 1956 NAS genetics report, saying that Jolly's dissertation

was compromised by his not noting information that came to light in the mid-1990s (Calabrese 2026 at pp. 464-466). Yet Calabrese has cited Jolly in 11 papers from 2009 through 2022 I found, which is an indication of how valuable Jolly's work has been to him in the past. I also found Jolly's broad coverage of all the players of the day, including physicians, government leaders, funders, various committees and the Atomic Energy Commission (AEC), to be very helpful background information. Calabrese now trashes Jolly for not covering in 2003 the issue of mosaic mutations in the mouse,'[25] which first came to light in a publication by Selby in the late 1990s. Calabrese neglects to point out that the first time he connected mosaic mutations to the 1956 NAS genetics committee was in 2025 (Calabrese and Selby 2025). Jolly was supposed to foretell the future? Yet, Calabrese states:

> "Beyea built his scientific beliefs on the compromised Jolly dissertation. The paper of Beyea (Beyea 2024), which depended on the flawed analysis of Jolly, continues to promote and propagate significant errors… " (Calabrese 2026 at p. 465).

This claim of a compromised dissertation is not only ridiculous but has nothing to do with my 2025 reliance on Jolly (and Beatty) for the fact that only seven of the nine geneticists calculated *total* damage. This number of nine seems to agree with Calabrese's

[25] "Jolly's dissertation has a profound omission, which amounted to a major error that his dissertation committee failed to detect and which Jolly, therefore, failed to address. Jolly failed to report that the Russell (Russell and Russell, 1997) (Russell and Russell, 1996) massive mouse radiation mutation study significantly overestimated mutation risks since the control group had a very low mutation rate due to underreporting large numbers of cluster mutations." At page 9 of Calabrese EJ. Confessions of an LNT Heretic: What Happens When You Challenge Biomedical and Environmental Dogma: Rebuttal of Beyea Commentary 2024. Health Physics 130: 456–500; 2026..

I note that the treatment groups also had low rates due to "underreporting," thereby keeping the slope of the dose response the same, which Calabrese neglected. The relative risk, however, would decrease, if mosaic mutations are considered appropriate for inclusion with the background rates.

latest paper on the subject (Calabrese and Selby 2026). I now think the seventh geneticist may have been Moser (submitted by Kaufmann), which was also recently suggested by (Calabrese and Selby 2026 at p. 13).

Moser did not provide uncertainty bounds, which would have separated him from the six others, for whom bounds were available.[26] Uncertainty bounds were one of the criteria set in the 1956 NAS report for the total damage calculations. I did rely in Part 1 on Beatty as discussed above for the idea that Sewall Wright would not be party to the activity, which was computing total damage. In my 2017 supplement, unlike in 2025, I relied on numbers from Jolly's thesis and numbers from Calabrese and Beatty. I also presented several quotes from Jolly in my 2017 supplement.

As for the charge that I did not consider Selby and mosaic mutations in the mouse, the absence of which supposedly compromised Jolly's thesis, I dealt with mosaic mutations in detail in the supplemental digital content in my Part 2 paper, supplemental Text S-5. Had I mentioned in Part 1 the proposed content of Part 2, this furor might not have been as great. However, at the time of submission of Part 1, I was not sure of the full content of Part 2.

Calabrese has provided a service to those who study the historical scientific record by bringing out from obscurity the fact that mosaic mutations were not included as possible background contributors to the mouse data of the Russells. However, his claim that including them in the data would have made an important difference to the reports of the 1956 and 1972 NAS committees rests on his own interpretation and his own

[26] Calabrese claims that William Russell did not submit bounds either, but Crow presented some attributed to him. Why not for Moser? I have not seen any documentation, nor has Calabrese provided any, about what exchange did or did not occur between Crow and Russell about the Russell bounds, nor about bounds for Moser, who was not a member of the committee.

calculations as to risk and those made with collaborator Paul Selby. I have challenged Calabrese's claims about the importance of mosaic mutations in my Part 2, Supplemental Digital Content, Text S-5, arguing that his math was wrong as to the radiation induced risk of genetic mutations, which in fact does not change. No threshold. No hormesis. Only a 2–3-fold reduction in relative risk,[27] which inclusion of mosaic mutations does produce using 1996/1997 quantitative calculations of the Russells. This decrease would not have likely changed any views, I believe, although a few lines in the 1956 NAS report might have discussed the reduced relative risk. There was disagreement among members of the 1956 committee[28] as to the importance of the background term by those who thought it irrelevant and focused on the rate of mutations induced by radiation, in contrast to those who thought relative risk put the induced risk in perspective.[29] My own view is that both relative and absolute risk are helpful measures to consider in risk assessment. I want to see both. And, in fact, the 1956 NAS report included both types of estimates,

[27] Paul B. Selby has argued for an increase in relative risk of 5 or more, but that claim would not have been available for consideration by either the 1956 or 1972 NAS committees. Again, the absolute risk per unit dose, i.e., the number of mutations above background rates per unit dose, would not change, despite Selby and Calabrese's claim to the contrary (citation), as I discuss in Part 2, Supplemental digital content, Text S-5.

[28] Quoting from the Conference on Genetics, United States Atomic Energy Commission, 19-20 November 1954, p. 77, Jolly writes on page 295: "At the conference, Beadle sided with Russell, arguing that while the spontaneous rate was a given, the induced rate from additional radiation was something that could be controlled and that, "I still insist that the amount you add is important irrespective of what you had to start with. That is, if half the people died spontaneously, and you killed half of the remaining ones, this would be important." Other participants, including Plough and Neel, argued that the relative amount of added genetic injury was important for putting the problem in its proper perspective, both for weighing the relative importance of radiation-induced genetic injury against other genetic hazards, as well as weighing the genetic risk against the benefits gained by testing, etc." Jolly JC. Thresholds of uncertainty: radiation and responsibility in the fallout controversy. Available at: https://ir.library.oregonstate.edu/xmlui/handle/1957/30847. Accessed 13 June 2024. In: Hist Sci. Oregon State University; 2003..

[29] If mosaics should happen to be produced by ionizing radiation, then the risks per unit of radiation would increase, not decrease.

despite arguments in committee meetings as to the importance of relative risk as recorded in the transcripts, which I discuss in Part 2.

8) Investigator bias?. Unlikely.

In yet another example, of inaccuracy in the video series, consider the claim that a report by Uphoff and Stern stated there was investigator bias in their work. The actual statement did not say that there was investigator bias in their work and it only spoke of the earliest control value, not the total:

> "The earliest control value is particularly low, and the question may be raised whether at this initial stage of the project it may reflect a personal bias of the experimenter" (Uphoff and Stern 1947)

Cardarelli muddies the waters in an attempt to dismiss my pointing out that Calabrese in his papers only quotes the ending of the (rhetorical) sentence from the report and leaves out the response to the question, which was, "It is unlikely that this is the case" (Uphoff and Stern 1947). Cardarelli focuses on a different sentence, which begins with their words, "It is hard to conceive of a bias."

> "The other half Beyea refers to is, "It is hard to conceive of a bias," which Beyea claims negates **Stern's and Uphoff suggestion that they were biased**." (Cardarelli 2026 at p. 14). (Bold formatting added.)

That is not the other half to which I referred. And Stern and Uphoff did not suggest they were biased. This is an outrageous distortion of their words. They said it was unlikely that there was a personal bias of the experimenter. They performed a statistical test. I confirmed this in 2017 with my own statistical test that showed the first control batch was not inconsistent with the variation of the whole group.

Cardarelli has misquoted both me and Uphoff and Stern in a way that makes the response to the rhetorical question seem innocuous. Cardarelli's rebuttal is to cloud what

was said in the Uphoff and Stern report by mischaracterizing Uphoff and Stern's words, not to admit that Calabrese made an error of omission and thereby falsely denigrated Uphoff and Stern with a claim of bias.[30]

It would be nice if Cardarelli could quote me and the historical authors correctly, but most important is for him to get the historical record straightened out. However, this example makes it clear to me that when a correction comes from me Cardarelli has no intention of correcting the record as he promised to do to viewers.

9) *Caspari's 1949 correspondence in support of Uphoff's replication of his work was not noted by Calabrese.*

As another example of Calabrese omitting contradictory correspondence, I cite a 1949 letter from Caspari to Stern about Uphoff and Stern's draft paper targeted for *Science (Stern 1949)*, which replicated Caspari's 21-day experiment with fruit flies (Caspari 1949). Caspari had consulted on replication of his experiment, spending three days at the lab at Stern's request as Uphoff moved to start the 21-day replication (Caspari 1947, Stern 1947). He wrote approvingly about Uphoff's results,

> "The data certainly settle two questions: There is no effect of aging on mutation rate and there are obviously mutations with chronic irradiation at 2.5 r per day." (Caspari 1949).

Caspari went on to urge that,

> "Generally speaking, this seems to be a most important paper and we both feel that it is most important that it be published as soon as possible." (Caspari 1949). (The other person urging publication was Warren Spencer.)

[30] The other error that Calabrese made was to think that the investigator bias statement applied to the whole control group, and not a fraction of it. Uphoff, like Caspari, did the experiment in batches. It was only the control for one batch that was unusual. Turns out that its variance was simply statistical fluctuation, as I checked quantitatively in 2017 using a different statistical test.

Thus, the idea that there was something wrong with Uphoff's data is not supported by anyone in the Stern group of experimenters. Unlike Calabrese, they did not ask for more details, knowing that she had copied Caspari's design with one modification that made no difference, and that she had guidance from Caspari at the start of the replication.

Caspari did note that the chronic mutation rate was lower than the acute rate, what would be called today a dose-rate effectiveness factor, which I estimated in 2017 to be about a factor of about two (Beyea 2017 at p. 368).

10) Cardarelli's dose-rate threshold is inconsistent with epidemiological data.

Calabrese does not make any quantitative proposals as to dose rates, but the HPS authors do, saying that adverse health effects are observed only above a rate of 50 mSv per year, as quoted above. This is not something I saw in the video series nor in any of Calabrese's historical papers. It is an extraordinary claim to make about cumulative dose effects, requiring rejection of recent studies of protracted exposure in humans, such as INWORKS, Techa River, and Mayak. Cumulative doses in those studies were built up from dose rates less than 50 mSv y-1, but show excess cancers. Cardarelli denies these studies, going back ten years, before the recent epidemiological data, to get a critical claim about my arguments about cumulative dose response:

> "Sacks & Siegel (2016) stated that "Beyea and other LNT advocates "employ hidden circular reasoning to arrive at their epidemiologic conclusions that increasing cumulative radiation doses cause increasing rates of cancer."

Below I show a dose-response graph from the latest INWORKS study (Richardson et al. 2023). After 1, 2, 3 and 4 years of 50 mSv/yr, a worker would receive 50, 100, 150, and 200 mSv dose, respectively, which are cumulative doses that clearly show excess risk in

the chart with 90%-confidence error bars in this epidemiological study well above no response. There is no need to assume linearity to see that many points of occupational relevance are statistically significant for cumulative dose.

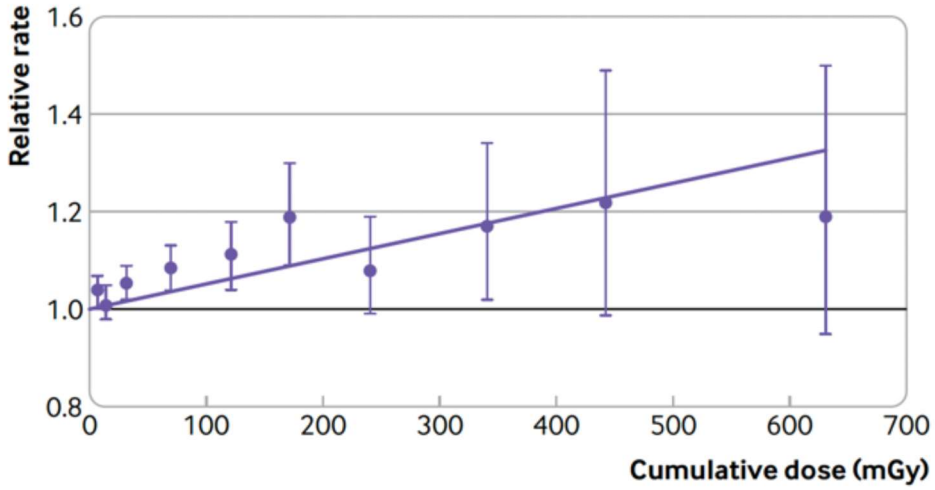

Figure 1. Increased relative rate of death from solid cancers in a population of 310,000 nuclear workers from France, the United Kingdom and the United States at an average age of 65.

Cardarelli is free to disbelieve INWORKS and the other large studies of protracted exposure that even by 2022 showed excess cancers at cumulative doses in the 100s of mSv like the studies at Techa River and Mayak, but he should have indicated his views were contested. Whereas there is a broad consensus that adverse health effects below 100 mSv acute doses have not been fully established, there is no consensus that health effects from a 50 mSv y^{-1} dose rate have not been demonstrated. A 2019 Health Physics position paper, PS010-4,[31] is more circumspect than Cardarelli:

[31] https://hps.org/wp-content/uploads/2024/12/radiationrisk.pdf

> "Epidemiological studies have not **consistently** demonstrated adverse health effects in persons exposed to small (less than 100 mSv) doses protracted over a period of many years." (Bold formatting added.)

As the president of the society at the time, writing about a production of the society, he should not have gone beyond the HPS position statement. This is another indication of his acting as if he could set policy for the HPS because he was president. The most striking indication was his statement in the rebuttal that, because he was president of HPS at the time, he did not have to include in the article a disclaimer to the effect that his article endorsing the Calabrese views did not necessarily reflect the policy of the HPS (Cardarelli 2026 at p. 425). In fact, his being president at the time was all the more reason for having such a disclaimer to ensure readers did not take the article as HPS policy.

11) Was Calabrese's reputation damaged?

Calabrese worries that I damaged his reputation. He worries about his own reputation but not the reputation of the historical geneticists, such as Curt Stern, Hermann Muller, Delta Uphoff, who he has tarnished by accusations of misconduct or bias. I do not accuse him of misconduct; I accuse him of research errors, specifically overlooking key historical text and making statistical mistakes, which have led him to false accusations. Calabrese has many talents, but historical objectivity and balance do not seem to be part of the mix.[32] In any case, I want to clean up some language in my 2025 report that faulted him for the wrong reasons about his claim of scientific misconduct on

[32] There are exceptions, where he writes about the history without taking a good-guy/bad-guy approach. See, for instance, Calabrese EJ. Origin of the linearity no threshold (LNT) dose–response concept. Arch Toxicol 87: 1621–1633; 2013.

the part of the NAS committee that prepared the 1972 BEIR report. I had implied that his claim was based on him saying that the committee ignored the dose rate dependency of genetic mouse data in retaining the LNT model. I should have said that his reason for the misconduct charge was that the 1972 committee ignored female mouse data, relying only on the male mouse data."[33, 34] Calabrese was wrong about the exclusion of female data, because the Committee included the female data (approximate zero response) as part of their average over sexes.[35] A sex average was an appropriate choice for application to the general population. Calabrese was explicit, saying that the committee *avoided*.[36,] and *rejected*[37,] the use of the female mice data, which suggests to me a charge of falsification

[33] "Nonetheless, the BEIR NAS Committee retained the LNT model, **relying only on the male mice data**."Calabrese EJ. Troubled History of Cancer Risk Assessment. 2019b.. Also: "They decided to construct a linear dose response from the lowest dose tested (i.e., dose associated with the ~70% decrease in mutations in males) to the origin. Calabrese EJ. The linear No-Threshold (LNT) dose response model: A comprehensive assessment of its historical and scientific foundations. Available at: https://www.sciencedirect.com/science/article/pii/S0009279718311177. Accessed 13 June 2024. Chemico-Biological Interactions 301: 6–25; 2019a..

[34] "Furthermore**, the decision to base the LNT recommendation on the male mouse data of Russell** is now seen as flawed (Calabrese, 2017a,b), providing no support for the BEIR (1972) decision in favor of LNT." Calabrese EJ. The Muller-Neel dispute and the fate of cancer risk assessment. Environ Res 190: 109961: 1–11; 2020. p 10

[35] "5. If the sexes differ in sensitivity, use the unweighted average of data for the two sexes." At page 51 of NAS. Effects on populations of exposure to low levels of ionizing radiation. Available at: https://nap.nationalacademies.org/catalog/18994/effects-on-populations-of-exposure-to-low-levels-of-ionizing-radiation. Accessed 13 June 2024. Advisory Committee on the Biological Effects of Ionizing Radiation, Division of Medical Sciences; National Research Council, National Academy of Sciences; 1972.. This is what the committee did with the mouse data on p. 52: "A chronic radiation dose to mouse spermatogonia produces about 0.5 x 10·7 recessive mutations per rem per gene. The reproductive cells of the female, for most of their lifetime, are very much less mutable than those in the male, even from acute irradiation… Therefore, we take the average of the two sexes as 0.25 x 10·7… "

[36]" It did so while **avoiding** the use of the Russells' female mice data that demonstrated a striking threshold effect (at 27,000-fold greater than background) and defaulting to the male mouse data that suggested (but did not quite achieve) a threshold, thereby preserving the LNT mantra of the BEAR I Genetics Panel. Calabrese EJ. Linear non-threshold (LNT) fails numerous toxicological stress tests: Implications for continued policy use. Chemico-Biological Interactions 365: 110064; 2022..

[37] "Therefore, the BEIR I Genetics Committee, with W.L. Russell as a member, selected Russell's male mouse data, **rejecting** his female mouse mutagenicity findings that had displayed an exposure threshold

of the research record, i.e., scientific misconduct, not a scientific mistake. If that was not enough, Calabrese also charged the BEIR I committee with misleading the scientific community[38] and deliberate misrepresentation of the facts[39] about its predecessor committee, the 1956 NAS genetics committee in connection with the Neel A-bomb data. Misleading readers and deliberately misrepresenting the scientific record certainly qualify as scientific misconduct. Yet, Calabrese in rebuttal claims he never accused the 1972 Committee of scientific misconduct:

> "This statement by Beyea is alleging that I accused the BEIR I Committee (1972) of scientific misconduct. He provides no evidence to support such an allegation. While it is true that I made such an allegation concerning the 1956 BEAR I Genetics Panel, this was never the case for the 1972 BEIR 1 Committee." (Calabrese 2026 at p. 460).

He never actually uses the words, scientific misconduct, but his words are equivalent. Also, I should have provided the citations that I provided here.in order that Calabrese could have contested my claims and others could have checked my citations. Nevertheless, my allegation was correct despite my giving the wrong reason in Part 1.

27,000 times background."Calabrese EJ. The Muller-Neel dispute and the fate of cancer risk assessment. Environ Res 190: 109961: 1–11; 2020..

[38] "**BEIR I misleads the scientific community** with regard to BEAR I ignoring human genetic studies." He goes on to say, "This seems to be an outstanding deception for which there is no supportive data, only much data showing that there was no such review. Furthermore, with a 16-year period of historical hindsight and the availability of the multiple Neel and Schull publications, this statement by the BEIR I Committee (1972) is **extremely misleading, if not patently false**." Ibid.. Misleading the scientific community would be falsifying the scientific record.

[39] **"**For example, in 1972 the BEIR I Genetics committee stated that the BEAR I Genetics Panel based its recommendations principally on Drosophila because there were no adequate human data available at that time. **This statement is a deliberate misrepresentation of the facts.**" Calabrese EJ. Ethical failings: The problematic history of cancer risk assessment. Environ Res 193: 110582; 2021.. A deliberate misrepresentation of the facts would be falsification of the record.

In addition to claiming that the entire 1972 NAS committee misled the scientific community, rejected valid data, and misrepresented facts, which would be scientific misconduct, Calabrese has suggested that a member, James Crow, continued his supposed misconduct on the 1956 NAS genetics committee during his tenure as chair of the genetics subcommittee for the 1972 NAS report. Crow supposedly did so by blocking any detailed exploration of the "historical dishonesties" of the 1956 NAS genetics committee[40] and by directing his subcommittee to obtain the desired LNT outcome.[41, 42]

Since the posting of the 22-part video series, Calabrese has claimed that a type of background mutation in mouse data was deliberately excluded by William Russell out of funding self-interest, constituting data suppression and deceptive practices[43] that led to

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[40] "For example, James Crow, a member of the BEAR I Genetics Panel (1956) chaired the Biological Effects of Ionizing Radiation (BEIR) I (1972) Genetics Subcommittee. This leadership would also effectively discourage any detailed exploration of **their historical dishonesties**." Calabrese EJ. LNTgate: The ideological history of cancer risk assessment. Toxicology Research and Application 1: 2397847317694998. Available at: https://doi.org/10.1177/2397847317694998 Accessed Jan 14, 2018; 2017a.

[41]" However, despite these new challenges to the LNT model, the Genetics Subcommittee still had a strong disciple of Muller in charge with Crow[8] and would find some rationale to keep the linear dose response model as the default if possible." Calabrese EJ. From Muller to mechanism: How LNT became the default model for cancer risk assessment. Environmental Pollution 241: 289–302; 2018..

Calabrese went on a page later to say. "These above noted questions were not explored or debated by the BEIR I Genetics Subcommittee. The point here is that the Genetics Subcommittee failed to broadly consider the question and were **directed by the Crow leadership to obtain the desired outcome**." Ibid..

[42] "However, despite such an acknowledgement, the BEIR I Genetics Subcommittee (1972) would not veer from the LNT recommendation of the BEAR I Genetics Panel, again showing its long term impact and the sustaining leadership of Crow." Calabrese EJ. LNTgate: How scientific misconduct by the U.S. NAS led to governments adopting LNT for cancer risk assessment. Environ Res 148: 535–546; 2016.. And on Page 4: "The issue here however is that the Muller, Crow and others of the original Genetics Panel found ways to extend their influence, casting a long shadow."

[43] "..deceptive/self-serving decisions.." in abstract. "…distortions of the scientific record.." at p. 2. "This strongly suggests that L. Russell knew about the problem and its experimental and risk assessment implications and that the Russells remained silent on this important issue. Because remaining silent would have sustained/preserved their scientific reputations, their doing so may be considered **self-serving and**

the masking of a threshold and misled the 1972 committee into recommending no threshold.[44, 45] Previous to 2023, Calabrese only claimed error on the part of Russell, which would not constitute scientific misconduct. As stated earlier, I argue in my Part-2-paper, based on data-fitting calculations and conceptual arguments, that no valid threshold inference follows from inclusion of these mosaic mutations, contradicting Calabrese.

It is difficult to keep track of which parts of the NAS, whether it be leadership, its committees or individual committee members, that Calabrese has or has not accused of misconduct. The two appendices that follow present my section-by-section response to

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consistent with their other deceptive practices." At p. 7.

"It seems likely that if W. Russell had shared his actual data with the BEIR I (NAS/NRC, 1972) Committee, rather than keeping the complication related to masked mosaics hidden, his findings would have been viewed as being strongly supportive of a threshold interpretation for both male and female mice." At p. 9.

"The implications of this **data suppression** by the Russells in retrospect were enormous because they improperly led to the LNT recommendation by the BEIR I (NAS/NRC, 1972) Committee and its later adoption by US EPA (1977)…" Selby PB, Calabrese EJ. How self-interest and deception led to the adoption of the linear non-threshold dose response (LNT) model for cancer risk assessment. Sci Total Environ: 165402; 2023..

"Thus, it was in W. Russell's **self-interest** not to share this information with others." at p. 11. "These collective findings indicate that EPA and other regulatory agency policies and practices worldwide are based on flawed and **corrupt scientific practices**." At p. 11.

[44] **"**This was a relevant question, for if the Russells had provided accurate control group information, it would have been available for the BEIR I Genetics Subcommittee through their 1970–1972 meeting period. In a recent paper, it was shown that if the Russells' upward correction had been made at the time of the BEIR I, the data would have revealed that the male mutation incidence at the low dose-rate would have displayed a threshold (i.e. the 70% decrease in mutation would be 100% with the error correction) [24,25]. If this had been the case, then the argument used by the BEIR I Genetics Subcommittee for the adoption of the LNT would have been invalid." Calabrese EJ. Troubled History of Cancer Risk Assessment. 2019b. at p. 19.

[45] "Had the Russell correction been available to the BEIR I [65] Committee, the LNT model may not have been recommended or adopted by the EPA since the corrected data directly refuted the scientific basis upon which the Committee had issued its LNT recommendation." Calabrese EJ. Linear non-threshold (LNT) fails numerous toxicological stress tests: Implications for continued policy use. Chemico-Biological Interactions 365: 110064; 2022. of ahead-of-print version.

the Cardarelli and Calabrese rebuttals, which are in turn followed by an Errata for my Part 1 Forum article.

Table 2: Overlooking by Calabrese of key historical text in primary documents[a)]		
Documents and context	**Misuse or misinterpretation**	**Significance**
(Uphoff and Stern 1947, Uphoff and Stern 1949)		
a) Investigator bias (1947)	Calabrese ignored, "It is unlikely to be the case."	Allowed Calabrese to claim that Uphoff and Stern had said they were biased.
b) Interpretability (1947 & 1949)	Calabrese Ignored that the lack of interpretability did not apply to 1949 study	Allowed Calabrese to claim that their key 1949 study was not interpretable.
c) Caspari attitude towards Uphoff study (1949)[b)]	Calabrese Ignored correspondence between Stern and Caspari, which showed he had helped Uphoff with her study, accepted her results, and wanted the study published as soon as possible.	Allowed Calabrese to suggest that Caspari was distressed by her study and Stern's acceptance of it.
(Muller 1946)	Calabrese ignored the "We believe" framing of the quotation that Calabrese claimed was a lie.	Allowed Calabrese to say that Muller had hidden the Caspari results and therefore lied.
(Furth and Upton 1954)	Calabrese ignored or didn't see the Figure for leukemia in mice going down to 16r, labeled Diagram 1, which "seem to allow the possibility of a linear no-threshold function but not to prove	In Episode 17, Lewis is called dishonest for removing the Furth reference that Calabrse falsely claims showed thresholds. See my part 2 supplement, item 10 in the Lewis Appendix.

	it." (Brues 1958 at p. 695).	
(NAS 1956a)		
a) Total mutant calculation by six geneticists	Calabrese ignored the fact that the NAS report had limited the calculation to *total* mutations and with uncertainty range	Allowed Calabrese to claim misconduct on the basis that the calculations of three more geneticists had been hidden, falsifying the record, when in fact their estimates did not meet the criteria.
b) Neel's A-bomb genetic data (1956)	Calabrese ignored the use of the A-bomb data in the report, e.g., "The experience in Japan gives some basis for thinking that the doubling dose is larger than 5 roentgens."	Calabrese claimed misconduct falsely stating that the NAS report did not account for Neel's A-bomb data, which Calabrese apparently thought showed a threshold.
c) LNT discussion[b)]	Cardarelli was not aware that the LNT was taken as a given in the agenda for the first meeting and no threshold was discussed repeatedly at the start with no objection to the agenda or discussion.	Cardarelli claims that the acceptance of the LNT was not done until after the Weaver offer to go to bat for funding it the case was made in the report. He misinterpreted this primary document.
d) Use of geometric mean[c)]	Calabrese used arithmetic error estimates. Cardarelli was not shown the multiple letters where committee members discussed the geometric mean, including an admission by Muller that in one calculation he had mistakenly used the arithmetic mean. See Item 40 in Appendix 1.	Cardarelli defends Calabrese's misconduct charge based on Calabrese's arithmetic calculation of an uncertainty of 750 in the challenge calculations rather than the committee's geometric standard deviation range of 100 (still very large). He says Beyea provided no basis for geometric calculations, which I

		must say are standard when dealing with a calculation involving multiplication of many variables, as was the case here.
(Neel 1994)	Calabrese, who read Neel's autobiography, ignored the positive words that Neel expressed for the 1956 NAS genetics report.	Allowed Calabrese to maintain a fiction that Neel's work was ignored by the 1956 committee, thereby claiming they falsified the record.
British report (MRC 1956)		
a) Auerbach review[b,d)]	Calabrese and Cardarelli left out the next sentence after the quote from Auerbach and did not include her quote about the two studies being very similar. See Item 29 in Appendix 1.	Calabrese uses the incomplete quotations to make it seem like the British differed from the US team on the science. If they had agreed on the science, it would be hard for Calabrese to argue that the US committee had falsified the record.
b) Matching dose recommendations	Calabrese never quotes the US committee's 50 R dose limit to age 30, nor compares it to the same limit proposed by the British team. Nor does he compare the US committee's recommendation of 100 R to age 40 to the similar British recommendation of 200 R in a lifetime.	Allows Calabrese to falsely claim that the two committees differed on recommendations. See Item 29 in Appendix 1.
c) The finding of a radiation induced difference in sex-ratio	Calabrese left out the fact that the British team claimed a radiation effect for sex-ratio in Neel's data. The US committee did not go that far.	Allowed Calabrese to suggest that the British team accounted for Neel's data. He then implied incorrectly that they found no genetic radiation effects at Japan.

Review of Selby charges (Mendelsohn et al. 1996)	Calabrese and Selby have left out key conclusions by the investigating committee set up by Oak Ridge that found no wrongdoing on the part of William Russell, although they said it would have been better if he had dealt with mosaics earlier.	Calabrese and Selby have falsely made it look like the investigating committee confirmed Selby's ethics charges. I discuss this in Part 2.,
(RERF 2024)	Cardarelli left out the sentence in the RERF report that said finding no effects in the A-bomb genetic data was not surprising since the average dose was only 11 rem.	Cardarelli used his incomplete information to suggest there was no impact from the A-bombs and that his and Calabrese's interpretation of threshold in A-bomb genetic data was correct.
a) Citations for most of this material can be found in Parts 1 or 2. Exceptions are separately footnoted, listed as new information or linked to the Appendices. b) This discussion is new information, not covered in either Part 1 or 2 c) Citations given in Item 40 in Appendix 1. d) See discussion in Item 29 in Appendix 1.		

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Appendix 1. Annotations by Jan Beyea of the Cardarelli rebuttal to Beyea's 2025 article in *Health Physics*. Version 2, 2 July 2026

In what follows, I place columns from Cardarelli's article on separate pages so that I can place my responses on the opposite column. I underline the Cardarelli words to which I am responding. My responses are italicized. Any page numbers left are not reliable. However, Cardarelli has an item numbering system that can be used for navigation.

Cardarelli repeats many arguments. His main response, repeated 56 times, is that Calabrese uses primary-sourced documentation, which supposedly makes Calabrese's interpretations unassailable.

He claims incorrectly 22 times that, unlike Calabrese, I do not use primary sources, opting for use of secondary sources (interpretations by historians) to challenge Calabrese.

Given this repetition, I have to repeat my counterarguments a boring number of times, particularly that:

1. Reliance on primary documents or any type of document is only as good as the interpretation of the documents. Pointing to words in a document is just the beginning. "Historians should not misrepresent their sources (AHA 2023) at Sect. 3." Cardarelli's defense is that he asked for documentation, assuming that he, a non-historian, could not be misled as to interpretation. He had no second source other than Calabrese.
2. Omitting contradicting evidence within documents that run counter to an analyst's interpretation is bad practice (AHA 2023) at Sect. 3. These omissions led to false premises on which claims of misconduct were based. This was my major critique of Calabrese in my Part 1 Forum article. The title includes, "overlooking of key historical text," which Cardarelli avoids in rebuttal, preferring to respond with examples of his looking at documents shown to him by Calabrese.

False and Misleading Statements Leading to an Unreliable Source of Information Regarding Early Research into Radiation Dose-Response: Part 1.A Response to Beyea

John Cardarelli II

Abstract **A commentary written by Jan Beyea claimed that the HPS interview of Edward Calabrese on the historical evolution of the linear no-threshold model was unreliable because it overlooked key historical text and statistical concepts. Beyea states that the purpose of his commentary was to defend the integrity of historical figures and committees from the accusation of scientific misconduct as presented by Calabrese. Based on his review of the video series and other documents, he provided what he defined as evidence of errors of fact, reasoning, and statistics to support his position. If true, Beyea's work would have the effect of impugning the reputation of Calabrese, myself, and the credibility of the HPS. This response intends to expose the issues with Beyea's commentary, including mischaracterization of Calabrese's work, lack of objectivity, misleading and factually incorrect statements, reliance on secondary sources, ignoring evidence specifically provided in the video series, and failing to address evidence provided in primary-sourced documents that contradict his conclusions. As a result, the reliability of Beyea's commentary is highly compromised, representing a serious lack of scholarship, research, and objectivity such that it should be retracted by *Health Physics Journal* based on the Committee on Publication Ethics guidelines. The HPS interview-style documentary reflects historical events based on primary-sourced documents as discovered by Calabrese. Scientific debate on this topic is necessary to progress our field, but the debate must be supported by facts with primary-sourced evidence and not driven by outdated public policies, logical fallacies, or ideology. Health Phys. XXX(00):00–00; 2026**

KEY WORDS: de minimis regulations; Health Physics Society; historical profiles; linear hypothesis; cancer risk assessment; LNT

INTRODUCTION

During my term as President of the Health Physics Society (HPS, 2021-2023), I led an effort to inform the

Cardarelli is concerned about his own reputation and that of Calabrese's, but not the reputation of the historical figures impugned by Calabrese. The way to deal with errors made in videos or papers is to correct them, not to deny them. That is the approach I take here with my work.

I do not question Calabrese's character. And, yes, Calabrese does rely in part on primary documents, namely, those that he selects, but as I have shown he is prone to selective stakeholder perception, which leads him to omit evidence from documents that run counter to his interpretation of the history. Much of my work on this topic is related to pointing out his false premises and his overlooking of key text in primary documents. In general, I rely mostly on primary documents from the period. This has escaped Cardarelli, because like Calabrese he does not appear to know the definition of primary documents. True, I have relied in part on secondary sources, including interpretations of correspondence by historians of science, who have professional standards in their field, such as expressed by the American Historical Society. .

Secondary sources are typically those "subsequent interpretations of those former times based on the evidence contained in primary documents" (AHA 2023). In other words, works by historians. Calabrese, a non-historian, has a high burden to reach before contradicting historians. I gave an example of how he does not meet that burden in the introduction.

There is a universe of correspondence. I demonstrate below and in the introduction that correspondence not cited by Calabrese negates some of his major claims.

What good does it do to rely on primary documents, if they are interpreted wrongly? Calabrese overlooks key historical text in primary documents and misinterprets statistical results.

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Reminder: This debate is about history of science. Dr. Calabrese has no training in historical studies. He has never collaborated with a historian nor published his claims in a history journal. He is an amateur in this field (as am I). As for trying to pull rank, the historical figures Calabrese denigrates include four Nobel Prize winners and others with numerous scientific awards and credentials. As for his expertise in dose-response, it does not include statistics of experiments as I argue in my Part 2 paper.

Calabrese showed documents to Cardarelli, who is also not a historian, but incorrectly interpreted them, as I discussed in the introduction. Cardarelli is not an appropriate judge of historical materials and was not able to spot omissions of contradictory information.

scientific community about the historical foundations of the linear no-threshold (LNT) model used for cancer risk assessment (Cardarelli et al. 2023). Edward Calabrese agreed to be interviewed and share primary-sourced evidence on how the LNT model became the dominant approach for cancer risk assessment. He has more than 1,100 peer-reviewed publications, 12 substantial toxicology/risk assessment textbooks, is co-editor of 24 books, and is the recipient of numerous awards including the Marie Curie Prize for his work on hormesis. Most notably, as of 2024, ScholarGPS ranked him #3 in the world in the field of Environmental Health Sciences and #4 in the Dose-Response Relationships field for his lifetime accomplishments. ScholarGPS rankings are based on "the most productive (number of publications) authors whose work are of profound impact (citations) and of utmost quality (h-index)."

Twenty-two short videos were produced covering the origins of the LNT theory to a path forward based on the knowledge gained since its inception over 70 y ago (HPS 2022). The video series received worldwide attention, bringing awareness and adding to the scientific debate regarding the validity of the LNT model in low-dose environments. Calabrese provided historical documents identifying errors, scientific misconduct, and a lack of scientific integrity in the field of radiation science involving multiple Nobel Prize winners and elite leaders in the field of radiation genetics (Calabrese 2024). More recently, its release generated a flurry of email correspondences that uncovered a covert coordinated effort among contemporary leaders within the Environmental Protection Agency (EPA), National Academies of Science (NAS), and the National Commission on Radiation Protection and Measurements (NCRP) to protect and maintain the LNT theory in US policies and regulations (Cardarelli 2024a). The increased awareness also brought criticism from devout LNT believers that any exposure to ionizing radiation presents risks and should be controlled as

low as reasonably achievable. This debate is welcomed and decades overdue because those in power who have controlled the narrative supporting the LNT theory have ignored calls for an objective and transparent exchange (Cardarelli 2023, 2024a and b).

This commentary provides detailed responses to Jan Beyea's criticisms that the video series had "errors of fact, reasoning, and statistics." This response exposes his numerous shortcomings including mischaracterization of Calabrese's work, lack of objectivity, misleading and factually incorrect statements, reliance on secondary sources, ignoring evidence specifically provided in the video series, and failing to address evidence provided in primary documents that contradict his conclusions. The 74 responses are presented in order as they appear in his commentary and are as follows:

1. [p. 507] Beyea uses the term "theories" when describing Calabrese's evidence supporting his claim that scientific misconduct was carried out on behalf of Muller and others (e.g., NAS BEAR 1 committee).
   A. HPS Video Series Episode 12: "Was There Scientific Misconduct Among the BEAR Genetics Committee Members"addresses the evidence supporting this claim using primary source data. Beyea fails to address or discredit the primary source data — yet he claims this information represents a "theory."This is a factually incorrect and mischaracterization of Calabrese's work.
   B. The same term "theories" was also used by Bahadori in his attempt to dismiss Calabrese's discoveries of fact based on primary sources (Bahadori 2023). When someone dismisses a truth by labeling it as "just a theory," they are employing a logical fallacy often related to equivocation or loaded language. Here's how these apply:
      A. Equivocation Fallacy: This occurs when a term like "theory" is used ambiguously, exploiting the difference between its scientific meaning (a well-substantiated explanation) and its colloquial meaning (a mere guess). By conflating these meanings, the argument misrepresents the truth.
      B. Loaded Label Fallacy: This happens when a label, such as "theory," is used to imply something negative or dismissive without addressing the actual evidence or reasoning behind the claim. This manipulates perception rather than engaging with the facts.
   C. On the contrary, assuming that the label "theory" inherently diminishes the validity of the concept is itself a fallacy since labels do not determine truth or falsity. These fallacies undermine critical

I repeatedly show in the sections that follow how Cardarelli has been misled and how in response to some of my charges, he clouds the record, which is evidence that he has no valid response.

So-called evidence from a primary document applied to a false premise is irrelevant. Much of the material covered in the video series and in the article of Calabrese et al. but not dealt with by me in my Part 1 publication is dealt with in my Part 2 paper, particularly defenses of Edward Lewis and Willaim Russell, as well as the claimed J-shape leukemia dose response in data too meager in number of cases to allow any conclusions to be drawn.

Calabrese sets up false premises about the BEAR genetics committee, for instance, that the report did not use the A-bomb genetics data of James Neel and coauthor and that such data showed a threshold dose response, rather than an inconclusive response. Other false premises include the claim that the 1956 report did not present the illustrative calculations of some geneticists and that the report did not properly present the great uncertainty in the illustrative calculations.

Theory is a respectable word, as in the theory of gravitation, the theory of evolution, the theory of relativity, the theory of the big bang and the theory of the superego (Freud).

I did not say or intend to imply that Calabrese's work was "just a theory." I expressed my concern for the embrace by Cardarelli and his team of "the scientific misconduct theories of Edward Calabrese."
Calabrese has put together a group of ideas that form a theory. I think it is a false theory, but it is indeed a theory.

reasoning by focusing on terminology rather than the evidence supporting the claim. Calabrese's primary-sourced evidence represents factual events that have occurred.

2. [p. 507] Beyea states the HPS acceptance of Calabrese's work as "authoritative and trustworthy" is not warranted because he argues it is "not deserved" due to a lack of historical accuracy and absence of statistical analysis supporting it.
   A. HPS Video Series Episode 1: "Who Is Dr. Edward Calabrese" explains why HPS accepts the work of Calabrese as "authoritative and trustworthy" and the reason for the documentary. Beyea fails to mention this here or when it is stated in Cardarelli et al. 2023. Further, Calabrese's lifetime achievements mentioned in the introduction support why he was accepted by the HPS as an authoritative and trustworthy source.
3. [p. 507] Beyea makes a statement that "No disclaimer was made in the paper that their views did not necessarily represent the view of the HPS."
   A. No disclaimer was deemed necessary in the paper (Cardarelli et al. 2023) since I was the President and official spokesman for the HPS at that time. However, a disclaimer appears on the HPS website and at the end of every episode in the video series.
4. [p. 507] Beyea states the purpose of his paper was to defend "the integrity of historical figures and committees."
   A. His paper does not discredit any of the primary-sourced evidence provided by Calabrese. Instead, Beyea relies on secondary sources to support his position while failing to challenge the integrity and accuracy of Calabrese's primary-sourced information.
   B. When someone relies on secondary sources to support their argument while ignoring conflicting primary sources, this behavior can be described as "cherry-picking evidence" or confirmation bias.
   C. Similar criticisms of Beyea were provided by Cuttler (2016) for his failure to "challenge any of the analyses and conclusions" presented by Calabrese's work. Sacks & Siegel (2016) stated that Beyea and other LNT advocates "employ hidden circular reasoning to arrive at their epidemiologic conclusions that increasing cumulative radiation doses cause increasing rates of cancer." Their tactic is to divert attention from debating the actual effects of Low Dose or Dose Rate (LDDR) radiation to debating the state of mind of the scientists who originated the claim. Siegel et al. (2017) criticized Beyea for ignoring key data contradicting his beliefs about the validity of LNT at low doses.

To repeat: Calabrese does not always interpret his primary sources correctly; he selects subsets, ignoring contradictory sources (as I documented in the introduction); he uses them to defend arguments based on false premises. Despite Cardarelli's claim, I rely primarily on primary sources such as transcripts and early scientific articles and reports. I have heretofore restricted my use of correspondence to that discussed or summarized by historians or cited by Calabrese, which I have done because I am not trained as a historian. However, for these replies, I have made use of correspondence that I previously collected to show how Calabrese in his selectivity misses key text there, too.

Sacks and Siegel do not criticize my historical work; they eschew Calabrese type attacks and address the LNT on its scientific merits as of 2016. The argument made here by Cardarelli is a distraction from the Calabrese debate. Sacks and Siegel were concerned with cumulative doses in 2016. Today, there is sufficient data that no assumption about the existence of LNT need to be made to show cumulative effects above 100 mSv.

But Calabrese often misinterprets the primary evidence based on overlooking contradictory material, setting up false premises as I documented in Part 1 of my series and in the introduction.

Calabrese's historical work is a small part of his total body of work. His work is not supported by historians. He cites himself repeatedly, because he is the only one who accepts his narratives.

This is an incredible claim, namely that as President and official spokesman for HPS at the time, he can endorse Calabrese's claims in his article and set HPS policy. Note that the disclaimer in the video series comes at the very end of episodes, after the credits, in the last frame below the logo. Few would ever see it.

5. [p. 508] Beyea states, "The purpose of this commentary is to begin to provide sufficient evidence of errors of fact, reasoning, and statistics to show that not only is the Calabrese prosecutorial case irrelevant to the validity of the LNT but that the case for scientific misconduct falls apart."
   A. Beyea does not define "scientific misconduct" anywhere in his paper. Without a definition, he distorts the facts to create an argument that he believes justifies the actions of the 1956 Committee and Muller. Calabrese used the NAS Code of Conduct to justify his claim that scientific misconduct was done by the 1956 NAS Committee and by Muller. The HPS documentary shows this definition in Episode 12 (timestamp 17:30).
6. [p. 508] Beyea states that members are given professional credit for watching the HPS video series. However, they do not receive professional credit for studying the critiques that have been written about Calabrese's misconduct claims.
   A. This is a factually incorrect statement. According to the HPS Directory, Beyea has been a member of the HPS since 1981 (nearly 45 y). He should know that HPS does not provide professional credits. Only the American Academy of Health Physicists (AAHP) provides continuing education credits to Certified Health Physicists (CHP). AAHP is an independent organization from the HPS. Beyea is not listed as a CHP in the HPS Directory, but he should have known this after being a member of HPS for over 40 y.
   B. A member of the AAHP Continuing Education Committee contacted me on 18 April 2022 (within days of the video documentary release) letting me know that they started an application for preapproval for up to 10 Continuing Education Credits toward maintaining one's CHP status. They asked if I would be willing to be listed as the "trainer contact." I said yes.
   C. The following was published in the May 2022, *Health Physics Newsletter*: "Great news! Not only can certified health physicists (CHPs) view the fascinating and informative HPS History of the Linear No-Threshold Model (LNT) video series, but they also may earn 10 American Academy of Health Physics (AAHP) continuing education credits (CECs). Watch all 22 episodes, which describe the historical foundations of the current protection philosophy based on an LNT model for cancer risk assessment, and then use the preapproval lookup code (2022-04-21-185) when submitting your CECs for recertification by the AAHP. And if you're a CHP who's not yet a

I distort no facts. I show that Calabrese distorts what the 1956 report says, setting up false premises. I use "scientific misconduct" in its common meaning, namely actions that would be construed as misconduct by scientists. Calabrese himself does not refer to any definition of scientific misconduct in his talk in Episode 12 (Otter.ai raw transcript), nor in his scientific papers. He just makes accusations based on claims that I have contradicted, namely the 1956 NAS committee not using the A-bomb genetic data of Neel, understating uncertainty, and not presenting the results of those who wanted their calculations presented. See introduction.

The NAS code of conduct shown on a slide while Calabrese is talking in Episode 12 leaves out key parts of the conduct that Calabrese should read:

"... treat all individuals in the scientific enterprise collegially and with respect..."

"..promptly move to correct the literature when errors in their own work are detected.."

I have to concede that I did not know that the AAHP has been separated from HPS. I see that that the two organizations have overlapping leadership, as they do today, e.g, HPJ editor Brant Ulsh is the incoming president.

member of the HPS, we welcome you to the Home for Radiation Protection Professionals: become a member today at hps.org/join."

D. Beyea's comment should have been directed to AAHP and not HPS. It inaccurately suggests the HPS has a bias that harms the Society.

7. [p. 508] Beyea criticizes Cardarelli et al. for "misleading" the readers because support for the LNT is primarily based on more recent data. He references NCRP Commentary 27 as supporting evidence for his assessment.

A. Beyea failed to acknowledge that the purpose of that paper (and the video series) was not to discuss the studies supporting the LNT model (i.e., NCRP Commentary 27). The purpose was to document the historical evolution of how the LNT model became the default model for cancer risk assessment. If it had been about an objective evaluation of the LNT model, a completely different product would have been produced. His statements incorrectly suggest the video series was about the validity of the LNT model.

Cardarelli claims that the video series was not about the validity of the LNT model. Calabrese repeatedly trashes the LNT indicating his claim that it is not valid. A major point of Calabrese's is that there were supposed fallacies in the LNT that were ignored. The transcript of episode 22 lays out Calabrese's desire for a threshold dose (as a compromise with hormesis). Throughout the series he is acting as an anti-LNT activist, not a history professor. Cardarelli et al. also want to replace the LNT as they indicate at the end of their 2023 paper. They want to replace "the most fundamental assumption (that any increase in dose is an increase in risk) with a true null hypothesis (that there is no effect)." P. 7, Item 19. They want to apply "the LNT model only to the point where adverse health effects are observed (e.g., above 100 mSv or 50 mSv y-1)." P. 7, Item 19.

B. A critical review paper by Ulsh (2018) indicates a strong pro-LNT bias by NCRP.

C. Cardarelli's "Overt Scientific Bias…" paper exposed a clear bias within NCRP and NAS leadership, questioning the ethical conduct of those leading these organizations/committees (Cardarelli 2024a). The credibility of NCRP Commentary 27 is affected. Beyea's reliance on NCRP exposes his appeal to authority to support his position — a logical fallacy. This was discussed in Cardarelli's "Bias" paper.

Cardarelli joins Calabrese in charging ethical misconduct by groups and leaders with whom he disagrees. An assertion of moral superiority.

8. [p. 508] Beyea states, "One cannot ignore the history of the atomic bomb cancer data or the history of the last 20 y, when large sample-size studies of protracted exposure to humans were developed (Hauptmann et al. 2020). It is necessary to look at and argue about current data to evaluate the LNT, as well as to make sense of the limited datasets collected in the early days."

A. Episode 17: "Studies With a Surprising Low-Dose Health Effect" specifically focuses on this topic. Many of the atomic bomb studies published from 1955 to 1978 were presented, including a discussion of Neel's original study of atomic bomb survivors. Beyea failed to recognize the information presented in this episode or has chosen to ignore it.

I have long pointed out that Calabrese mischaracterizes Neel's original study. It did not show a threshold. And why did Calabrese stop at 1978 in presenting atomic bomb studies? What about the last 20 years of A-bomb data, with its larger and larger sample size of data on cancer incidence and mortality?

9. [p. 508] Beyea states that Cardarelli and "his co-authors put their trust in Calabrese's words without any apparent review by persons knowledgeable about the historical period or any direct reading of the contested historical reports."

A. Beyea makes a declarative statement as if it is true. This is a false statement. There were many debates and arguments with Calabrese during the production of these videos of which people are not aware. I required Calabrese to produce primary-sourced documents to support his opinions/statements. The documents shown in the videos represent anything that could be perceived as controversial. For example, Episode 6: The Birth of the LNT Single-Hit Theory (timestamp 11:07) shows the handwritten letter from Muller's PhD student expressing doubts of his work that Muller later referenced in his Nobel Prize lecture. In Episode 8: "Fly in the Ointment" (timestamp 7:30), Calabrese suggested that there was an effort to "save the hit theory" implying a motive of the early researchers. I strongly challenged Calabrese to provide irrefutable evidence using this language because of the serious nature of that interpretation. A letter is shown in the video highlighting these words that supported Calabrese's statement. Without that letter, that section of the interview would have been removed. There are many more examples throughout the 22 videos as well. Beyea had no knowledge of what went into making this documentary, but he attempts to discredit the videos by making unsubstantiated comments that he cannot prove. It is clear that he was factually incorrect in his statement.

No, I said, "..without any apparent review by persons knowledgeable about the historical period or any direct reading of the contested historical reports." This left Cardarelli an opportunity to contradict me, but he does not. He changes the subject, referring not to a contested historical report, but to a letter. Nor is Cardarelli a person knowledgeable about the period.

I repeat what I said in Part 1: There has been no review by persons knowledgeable about the historical period. Cardarelli does not qualify. He is like a member of a grand jury being shown material by a prosecutor who controls the evidence and its interpretation. In such a case, members are willing, as the saying goes, to indict a ham sandwich. Nor has Cardarelli indicated any direct reading of the entire contested historical reports, such as the 1956 NAS genetics report. He has put his faith in Calabrese's interpretations and repeated them at the end of each episode as learnings.

I saw the outcome and read the HPS article and there was no apparent review by persons knowledgeable in the period, which Cardarelli does not deny. Nor does he indicate that he read at the time the full 1956 NAS genetics report.

Cardarelli should have reread my 2017 paper during the making of the video series, to which Calabrese never responded, prime evidence that he could not.

Cardarelli refers to Initial 1947 data by Caspari, but omits the 1949 replication by Uphoff, which Caspari discusses in a 1949 letter not mentioned by Calabrese (see introduction). After reviewing her work, on which he had consulted, Caspari agreed that the data showed effects at 2.5 r per day. Moreover, Caspari urged that her paper be published as soon as possible (Caspari 1949).

The single hit theory was the dominant view among geneticists, based on the physics of radiation interactions and geneticists' visual experiences with counting chromosomal breakages under a microscope. The way to save the hit model was to do more experiments, which were done. Calabrese only believes the results of the first experiments, but that is a different issue, which I dealt with in detail in my 2017 paper, which Calabrese and Cardarelli have ignored.

10. [p. 508] Beyea makes three statements that he states Calabrese (and Cardarelli's paper) are asking readers to believe:
    A. "Researchers who did groundbreaking research before 1950 relied on data they knew to be faulty to reject a dose threshold model, thereby engaging in scientific misconduct";
        A. TRUE: See Episodes 3 to 12 and Cardarelli's "Response to Badahori"; They had a clear motive to "save the hit theory" (Cardarelli 2023a).
    B. "To increase funding for genetics and to support an ideology, a 1956 committee of the National Academy of Sciences engaged in scientific misconduct in support of dose linearity by suppressing genetic data from Hiroshima and Nagasaki and by suppressing disagreements among committee members about risk magnitude";
        A. TRUE: See Episode 11: "Creation of the Biological Effects of Atomic Radiation (BEAR) I Committee"; Episode 12: "Was There Scientific Misconduct Among the BEAR Genetics Committee Members," and Episode 15: "Follow the Money Trail: We Are Just All Conspirators Here Together." These videos present

A.

I have indeed provided both primary and secondary evidence to contradict Calabrese's interpretation of scientific misconduct. Ignoring what I say in Part 1 and have said in 2017 is not a good response.

1) The 1972 NAS report did not dismiss female mice genetic data; they averaged over the data for both sexes.
2) This claim by Calabrese that the so-called errors change dose response to threshold or hormesis is based I argue on a conceptual error that I document in my Part 2 paper's supplementary digital content. Even with his so-called corrections, the threshold response is not statistically significant.

No, demonstrably false.

No reason is given for Calabrese not responding to my 2017 paper. Cardarelli than quotes one place where I rely in 2017 solely on the writings of historians, but I show I do not need to, because I have numerical evidence in a primary document itself, the 1956 NAS genetics report. Very misleading.

James Crow was correct to separate estimates. Calabrese overlooks the stated requirements given in the text for the illustrative calculation. Demerec did not calculate total mutations in the first generation, so his number did not belong with the six estimates that made total calculations. The other two estimates also did not belong with the six. Wright calculated only serious mutations, not total. Moser did not provide the uncertainty bands as was a requirement listed for this illustrative calculation. Moreover, Moser's estimate was not low.

I show in the introduction based on correspondence that historian Beatty was correct and Calabrese was wrong. Cardarelli was shown a subset of documents and what he saw was misinterpreted.

the evidence that Beyea doubts. Beyea should have provided primary sourced evidence that conflicted with the primary sources presented by Calabrese.

C. "A 1972 NAS committee failed to take into account genetic mice data that showed dose-rate effects."
   A. TRUE: See Episode 20: "BEIR I Acknowledges Repair but Keeps LNT. Why?" and Episode 21: "BEIR I Mistake Revealed, LNT Challenged, Threshold Supported." These episodes summarize how the mice data were discussed in the 1972 NAS committee. They decided to dismiss the data that showed a threshold for females in favor of a linear response for males. Later, it was determined that these data had errors which, after correction, showed a hormetic effect in females and a threshold for male mice.
D. All three statements are demonstrably TRUE — supported by primary sourced documents presented in the HPS documentary series. It seems clear that Beyea's commentary is fatally flawed once these statements are shown to be accurate. This alone could be enough to retract his paper.

[p. 508] Beyea references his two articles in 2016 and 2017 critiquing Calabrese. Calabrese responded to Beyea's 2016 paper but chose not to respond to his 2017 paper. In Beyea's 2017 paper (page 372), he writes, "Based solely on secondary sources written by historians, I have not been able to figure out exactly which committee members' calculations went into generating the 50,000 tangible mutation estimate, but it is clear that Demerec's low estimate must have been one of them. There is no other way that the reported number could be as low as 50,000, because all other estimates that may have contributed were greater than 50,000" (Beyea 2017).

A. Episode 12: "Was There Scientific Misconduct Among the BEAR Genetics Committee Members?" (timestamp 9:15) clearly shows that Demerec's low estimate (5,200) was one of the three reports removed by James Crow in an attempt to lower the uncertainty by keeping the six highest estimates and removing the three lowest estimates. It also contributes to the scientific misconduct discussion.
B. In 2017, Beyea admitted he relied on secondary sources written by historians. He relied on these historians to be accurate, but they weren't, and his acceptance of their interpretation without confirmation constitutes confirmation bias on his part (Beyea later refers to this as "selective stakeholder

perception"). Calabrese's publications relied on primary sources. Many are shown in the HPS documentary.

C. Beyea also relied on secondary sources throughout his 2024 paper. He writes with such authoritative certainty most readers would tend to believe him. He was wrong in 2017 and wrong throughout his 2025 commentary.

12. [p. 508] Beyea wrote "Without mention of the contrary published papers, the viewers of the series and the readers of the article are not 'informed.' Rather, they are misled. A reader gets no sense that the scientific misconduct information is deeply contested."

A. The purpose of Beyea's paper was to defend "the integrity of historical figures and committees." The HPS series is a historical documentary of how the US came to accept and incorporate the LNT model; it was not to refute or support the scientific validity of the LNT model. Beyea makes a strong statement that this series misleads viewers because the scientific misconduct information is deeply contested. In fact, those contesting it rely on secondary sources, which have shown to be wrong. HPS required primary sourced data to support Calabrese's interpretation of events. He provided everything HPS requested for all 22 episodes. Beyea does not provide primary sourced documents to support his argument or contradict the documents provided by Calabrese. So if there is any misleading information being provided, it originates with Beyea.

13. [p. 508] Beyea argues that HPS took a prosecutorial position to posthumously convict historical figures who are no longer able to defend themselves of "scientific misconduct." This motivated him to write this paper.

A. What Beyea does not know is that the HPS offered to do a follow-up video to have "world experts" defend the LNT position or give an alternative interpretation of its history — none agreed to go on camera under the same conditions of Calabrese. (e.g., no notes, no script, must provide primary source documentation). Ironically, the NCRP videos describing how to interpret epidemiological data were scripted (admission to this effect can be provided via email correspondence from J. Boice). It also is worth noting that the two individuals in the NCRP videos were instrumental in working covertly to suppress the scientific debate about the validity of the LNT and take over the HPS (Cardarelli 2024a).

B. What is there to defend? The primary source documents are there in their own words (e.g., letters,

Calabrese ignored subsequent contradictory info in primary documents, as I pointed out. In both 2017 and in 2025 I described how I relied on both primary and secondary sources. Cardarelli overlooks what I wrote to come up with a false defense.

No. I rely primarily on primary sources, showing that Calabrese overlooked sections in the report of the 1956 NAS genetics committee, which is the most important primary document of all. When Calabrese or Cardarelli misstates or mischaracterizes what the report wrote, I hold them accountable for it.

In the introduction to the appendices, I showed from the raw transcript that I prepared of the video series that it was an anti-LNT production. The idea that "it was not to refute" the scientific validity of the LNT model is contradicted by the language used by Calabrese and endorsed by Cardarelli in the ending summary.

It is false to imply that I rely on secondary sources for my criticisms of Calabrese. I do only in small part and moreover, as I discuss in the introduction, the historians that Calabrese challenges, such as Beatty's views on Sewal Wright, were actually correct, when correspondence of Wright to Weaver were reviewed. It is necessary to look at all the correspondence, not just letters that contain something you like. It is the interpretation of primary sources that I say Calabrese got wrong.

HPS asked "world experts" to defend the LNT? I thought refuting or defending the LNT was not supposed to be the point of the series, as stated above. As for alternative interpretations of Calabrese's anti-LNT history, did you ask Carlson or science historians? I certainly was not asked.

What was the point of having Calabrese go on camera without notes and without a prior script? Notes and scripts help academics get their facts straight. It is actors who go on screen without notes. Calabrese himself complains in his rebuttal about the conditions he was put through.

The documents have to be interpreted correctly and a complete set needs to be reviewed.

Exactly what acts of mine were exposed in Cardarelli's bias paper? I critique Calabrese's use and interpretation and completeness of primary documents. From where does Cardarelli derive his moral superiority? Cardarelli has become like Calabrese, an accuser of others who do not support his positions. Smear campaign? Look in the mirror.

It is the number of Nobel Prize winners denigrated that is incredible, along with a dozen more major researchers on the 1956 Committee. Calabrese makes incredible claims about people who have been studied by historians. Incredible claims require incredible evidence, which Calabrese has not provided. That which he has provided I found to be wrong. Cardarelli did not tell viewers that Calabrese's claims are contested. Why not?

I have said that Calabrese's claims are not sufficient and I have shown repeatedly that his work is based on false premises derived from incomplete reading of primary documents. He overlooks contradictory text. In the introduction I give a new example of his ignoring contradictory correspondence from Sewall Wright to chairman, Warren Weaverl

Cardarelli's review of selected papers is not sufficient to validate Calabrese's interpretations of them. Cardarelli has been acting as a journalist in narrating the videos. Journalists usually require two sources before going public. Cardarelli only has one, Calabrese. No historian.

It is a lie and misleading to say I provide no primary source documentation. It is a lie, because the 1956 report and articles of the day are primary sources, they are not historians writing about those days. It is a misleading statement, because I show how Calabrese overlooks contradictory text in his own sources.

The deck was stacked. Calabrese was asked to provide primary sources to support his interpretation; I was not.

I did not intend to imply that the HPS leaders were engaged in character assassination. I wanted readers to know that Calabrese's work has been so labeled.

transcripts). Further, Cardarelli's "Bias Paper" showed how living people conducted unethical acts, but they have done nothing to date (August 2025) to defend themselves. Ignoring these facts is the modus operandi of those who have no defense — other than to attempt a smear campaign to discredit those who've exposed their acts. Beyea's commentary is consistent with this practice.

14. [p. 508] Beyea states that it's "an incredible claim for an organization's leadership to have made about three Nobel Prize winners (Hermann Muller, George Beadle, and Edward Lewis) and other prominent scientists, just because Dr. Calabrese, a critic of the LNT, says so."
    A. A Nobel Prize doesn't make one without question nor validate the science. The very essence of science is to question! When new evidence is found, science should evolve. Beyea suggests that these Nobel Prize winners are untouchable and that we must accept that their motives were always true and pure.
    B. As the President of the HPS, I recognized the statements made in the series could be (and have been) challenged and bring unwarranted attacks on me professionally and to the Society. Beyea's paper is an example of this. That is why I insisted on seeing and providing primary sourced documents for any statement made by Calabrese that could be considered controversial. See my response in 9(A) for more details.
    C. The information revealed in the series is "incredible." It's also worth noting that the LNT message has been the dominant message for nearly 70 y and efforts to challenge it have been met with ignoring, canceling, excluding, and shunning by the authoritative powers. See Cardarelli 2024a.
15. [p. 509] Beyea states that Calabrese and HPS authors "...have distorted the record, I believe inadvertently. If there is any 'shocking exposé,' as the HPS authors claim, I shall argue it is of their own work."
    A. Beyea's article is more representative of a shocking exposé because he provides no primary source documentations to support his claims. Yet, he expects readers to accept his version of history and reject Calabrese's version of history.
16. [p. 509] Beyea references a 2011 article ("Croc 2011" — which is actually spelled "Crok") to infer that the HPS series conducted "character assassination" and conducted "sloppy historical analysis."
    A. These are strong accusations that should require hard evidence. These are easily refuted. The Crok article stated that "Calabrese makes some scurrilous accusations, he accuses a dead man [Muller]

of being a liar. That's character assassination." In Episode 8: "Fly in the Ointment" (timestamp 12:50), Calabrese describes the evidence that exposed Muller as a liar. HPS accepted this statement based on the primary sourced materials he provided to support it. The fact that Muller was dead at the time is meaningless. Beyea should have seen this evidence in Episode 8 and sought his own primary sourced documentation to challenge the documents provided by Calabrese (and shown in the HPS series), but he didn't.

B. Episode 14: "Should the Genetics Panel Science Paper be Retracted" (timestamp 12:30) presents a similar story criticizing a deceased Nobel Prize winner (Robert Millikan) for scientific fraud. Beyea should have acknowledged this historical precedent in his paper. To label or suggest that Calabrese or the HPS conducted "character assassination" because the subject is dead is salacious and defamatory without providing evidence. Beyea infers it by accepting Crok's definition — which itself has many problems.

C. Beyea's failure to discuss recently published articles supporting the series represents his incomplete historical analysis.

D. Episodes 12, 15, and 21 all provide documents that support the "scientific misconduct" conclusion.

17. [p. 509] Beyea states that he could not replicate Calabrese's claims of misconduct yet attempts to explain what is needed to make this claim.

A. Beyea compliments Calabrese as a serious researcher in that he lists his sources in his citations. He claims he cannot replicate Calabrese's interpretation but does not attempt to refute Calabrese's interpretation by providing his own set of primary sourced documentation.

B. Instead, Beyea shifts the burden of proof away from the primary sources to his "statistical analysis" as providing the "guardrails against false interpretation of count data that can be caused by statistical fluctuations and uncertainties." This has nothing to do with scientific misconduct and obfuscates the issue of whether Calabrese is correct about his "scientific misconduct" conclusions. He attempts to redirect attention to whether LNT is supported by the science or not. That was not the purpose of the HPS video series.

18. [p. 509] Beyea stated in the abstract, "In reviewing a video series that they created for the website of the Health Physics Society…," suggesting he watched the video series as part of his review method. He also relied on word searches of documents he was able to obtain via internet searches. He used this method to

If accusations are largely true, they can be labeled an expose; if largely false and knowingly so, they can be labeled character assassination. I don't think Calabrese's work falls under the character assassination label, but I can understand others who do. I use other language in Part 3.

Calabrese regularly fails to include the "We believe" section of the Muller quote that makes his sentence true. Scientists regularly use the "I believe" caveat when their work is not certain or contested. They are not lying. Cardarelli dismisses this as nit-picking.

And Calabres builds his lying charge on a false premise. Calabrese is mathematically wrong to say that the 1948 Caspari and Stern experiment showed a threshold and Muller should have known that. The result was inconclusive.

There was no fraud committed by the committee; As I have documented, Calabrese overlooks text, especially in the 1956 report itself. He selectively spins content of correspondence and transcript excerpts, along with speculative mind-reading of his targets.

Episode 21 deals not with the 1956 NAS committee but with Russell and the 1972 NAS report. I deal with these claims in Part 2.

Calabrese does not get to define primary documents as his selection of documents. The 1956 report, the transcripts of the meetings and the early scientific literature are all primary documents, which I use to overturn Calabrese's claims. I also make use of his interpretations of primary documents. Primary documents need to be interpreted correctly and to be representative, not selective.

Statistical mistakes matter; they led Calabrese to false premises. Using primary documents to support accusations of scientific misconduct based on false premises is not a valid methodology.

The committee did use the A-bomb genetic data to set a lower limit in its report on doubling dose. Calabrese missed this.

More than the word, Japan, appeared in the report. The Report made use of the data from the early A-bomb genetic studies: "The experience in Japan gives some basis for thinking that the doubling dose is larger than 5 roentgens."

Thus, the committee did incorporate Neel's data to set a lower limit on the dose that would double the genetic mutation rate in humans. That is all the data would allow at the time. Calabrese is wrong to think that this inconclusive data could be used to support a threshold

If the committee did not review the Neel data and did not incorporate it in their analysis, then how did their quantitative use of it appear in their report? As I show in the Introduction, Sewall Wright discussed the actual data quantitatively in a letter to Chairman, Warren Weaver, providing analytical support for the statement that appeared in the report.

Muller was an influential member of a committee with disparate views, but he could only express his opinion. All he said in the midst of a long discussion about many other topics was, "be wary." Calabrese provides no basis for saying the committee was convinced of anything. The actual report text includes the data as part of the discussion of doubling dose. As the above language from the final report shows, Muller did not convince the committee to exclude the study by Neel, who was a member of the committee. And Neel, as I stated in Part 1, was happy with the Report as expressed in his autobiography.

Replacing current regulatory and cancer models with a no-effect null hypothesis (threshold) is a call for an anti-LNT change in regulatory policy.

check Calabrese's "claim that a 1956 genetics report by the National Academy of Sciences (NAS) failed to discuss the early atomic bomb genetics studies in Japan…." Beyea argued that since the word "Japan" was included in report, the committee must have discussed the early studies. Thus, he assumed his critique of Calabrese was correct that he "has no way of proving negative claims, such as what was *not* done or *not* said in a document."

A. Beyea failed to provide the full context of Calabrese's statements which is shown in Episode 11: "Creation of the Biological Effects of Atomic Radiation (BEAR) I Committee" (timestamp 20:15). Calabrese discussed how the BEAR Committee started to address the Japan study by James V. Neel but ultimately dismissed it. The BEAR Committee didn't review the study as part of their evaluation. Beyea did a word search and found the word "Japan" and must have concluded that they did discuss it. The committee did discuss the fact that they would not incorporate it into their analysis. Beyea writes "by the word's presence, along with the context, that the genetics atomic bomb data actually did inform the report." Beyea does not provide any evidence of the context that was discussed by the committee. Calabrese provides the transcript and HPS highlights (timestamp 20:35) where Muller convinced the committee to exclude Neel's study. The highlighted language by Muller states, "We should beware of reliance on illusionary conclusions from human data, such as the Hiroshima-Nagasaki data, especially when they seem to be negative." Beyea should have gained a better understanding of why the committee didn't address the atomic bomb study by Neel, as was clearly discussed in the series. Therefore, Beyea's effort to derail the content Calabrese provided in the documentary failed immensely. Such information, since it is unreliable, supports an argument to retract his commentary.

19. [p. 510] Beyea states: "A puzzling feature of the HPS authors'article is how the authors can leap to a call for changes to regulatory policy without consideration of the recent history of the cancer LNT."
    A. His statement is factually incorrect and misleading. Our paper did not call for changes to regulatory policy. It specifically states:
        A. Re-examine the current regulatory and cancer risk models by replacing the most fundamental assumption (that any increase in dose is an increase in risk) with a true null hypothesis (that there is no effect).
        B. Work toward an understanding of the low dose response using an integration of evolutionary

biology principles and **current epidemiological research findings**. (emphasis added)

C. Re-evaluate the regulatory paradigm of "as low as reasonably achievable" (ALARA) to determine its merit on a scientific basis; and

D. Harmonize radiation protection by considering all dose response models and applying the LNT model only to the point where adverse health effects are observed (e.g., above 100 mSv or 50 mSv y^{-1}).

To forbid using LNT below observable effects is clearly a replacement of the LNT. Calabrese only allows pure dose threshold or hormesis, no supralinearity, in his "optimization", ensuring a threshold is his "compromise.".

20. [p. 510] In reference to the BEIR VII report, Beyea states, "No controversial claims about low-LET radiation effects should be made without first reviewing what this comprehensive report has to say about the topic."

A. Beyea suggests this report represents the "modern LNT history." I published an article that answered the question, "Should the BEIR VII report continue to be used to justify the use of the LNT model for LDDR radiation environments?" (Cardarelli and Ulsh 2018). The answer was NO and a rationale was provided.

False premise. I said this 2006 NAS report should be reviewed first about a controversial claim because of its comprehensiveness. The modern history of the LNT, as I discussed, includes data on cumulative radiation exposure not available in 2006. INWORKS (2023), Techa River (2015), and Mayak (2021). These datasets contradict Cardarelli's claim of no effects at 50 mSv/yr regardless of the number of years accumulated. Cardarelli may not believe the results and he may be willing, in return for other social benefits, to impose the corresponding risks on nuclear workers that ALARA now reduces greatly, but his view is not universal.

B. Ironically, Beyea referenced that paper in a positive context on p. 509 where he notes our criticisms about medical exposures in the INWORKS study and how that criticism led to a 2023 update. That same paper says BEIR VII should not be used to justify the use of LNT model for LDDR radiation environments. Beyea seems to have ignored that section.

21. [p. 510] Beyea mentions the Russells' mice data and how it was not used in genetic risk assessment as described on page 96-97 of BEIR VII. Beyea also stated that "any hypothetical correction to the mouse baseline rate is irrelevant today, as it was in the 1972 BEIR I report, which also did not use the mouse spontaneous rate (NAS 2006) at pp. 12 and 123."

A. Episodes 19, 20 and 21 discuss Russells' work with mice and genetic repair and how the 1972 BEIR I report chose to handle it. Episode 21 (timestamp 22:00) discusses how the corrected Russell mice data would have likely changed the LNT recommendation. Beyea claims the correction was "hypothetical," yet the Department of Energy required the Russells to publish a correction to their results after conducting an ethics investigation into their past publications, which was exposed by Paul Selby (Russell, & Russell 1996, 1997). Calabrese and Selby (who worked with/for Bill Russell for decades) recently published articles that Beyea should have cited. Instead, Beyea makes a misleading statement by characterizing the necessary correction as "hypothetical," which is a factually incorrect statement.

Calabrese and Selby's alternative history is hypothetical. They make their own use of 1996 data to falsely claim a threshold dose response results, as I argue in Part 2. To imagine their supposed correction would have been accepted by statisticians at Oak Ridge is also hypothetical.

I show in Part II that Calabrese and Selby made a conceptual mistake assigning different background rates to treated and untreated mouse populations. Moreover, a threshold fit to even their "corrected" data does not give a significant result for dose threshold, I found.

The lack of mention of Muller, Sten, Uphoff, and Lewis in the 2018 NCRP document and others indicates their lack of relevance to the modern history of the LNT.

22. [p. 510] Beyea mentioned the "2018 NCRP report" and how it didn't mention Muller, Stern, Uphoff, Lewis, or Russell, nor genetic data as if that somehow negates their errors of the past, and infers that the 2018 NCRP Commentary 27 represents the latest science.
    A. Beyea incorrectly refers to the 2018 NCRP Commentary as a "report." In my 2024 "Bias" paper, I note the significant difference between an NCRP "report" and an NCRP "commentary." Beyea's use of the term "report" is misleading because it infers that the entire NCRP commission voted to accept it, when in fact a commentary only requires 11 of 13 members to approve it. Beyea must not have been aware of my recent paper or chose to ignore it.

I am happy with 11 of 13 approvals for the NCRP document.

    B. Beyea also fails to acknowledge a peer-review publication by Ulsh (2018) which was highly critical of NCRP Commentary 27. This represents his lack of scholarship to review the literature objectively, thereby misleading his readers into believing that the NCRP Commentary 27 was not contested, a criticism he's levied in his article about the HPS. The difference is that the HPS video series was not intended to discuss the pros and cons of the LNT model as Beyea seems to believe. The purpose of HPS video series was to educate viewers on the history on how it came to be — not to argue for or against it.

False premise. I stated only that "...support for the LNT is primarily based on more recent data, as exemplified in the 2018 report of the National Council of Radiation Protection (NCRP)..." This has nothing to do with whether or not the document is correct or the LNT is contested.

Wow. The HPS series is anti-LNT through and through. The name Calabrese is synonymous with anti-LNT. In the final episode Calabrese trashes the LNT and proposes his own "compromise" that is essentially a threshold model, with linearity above only.

23. [p. 510] Beyea discusses a few epidemiologic studies that didn't show a threshold effect at 50 R and then states, "Thus, the dose levels of these early animal studies are too high to be of any obvious use in resolving issues of dose or dose-rate threshold for cancer. This should have been stated."
    A. This limitation is discussed throughout the HPS video series, especially in Episode 3 (timestamp 7:30) which referred to mutations vs. cancer. Beyea stated that he reviewed the HPS video series, so he should have known that the HPS (via Calabrese's interview) noted this limitation in the application of the LNT.

Episode 3 has no mention of cancer by Calabrese. When he wants to criticize the LNT, Calabrese will point out that Muller's fruit fly doses were very high. I stated about modern epidemiological studies that, "There is no threshold evident in radiation-induced cancer at protracted dose levels comparable to the 50-R fruit fly genetic studies of interest to the HPS authors, and thus there is no threshold either at the dose levels in the mouse genetic studies they discuss." (The mouse genetic studies used doses much higher.)

The issue is why did the series not say that modern support for the regulatory LNT is based on modern data?

    B. I do not believe my response warrants a debate on which studies support vs. contest the LNT model for Beyea's paper to be retracted, although it is worth noting that his referenced papers can be easily countered.

24. [p. 511] Beyea states, "If anyone had tried in 1972 to follow Calabrese's hindsight approach for cancer, they would have been contradicted by now."
    A. The doses that Beyea is referring to are 37.5 R and 86 R to mice — not humans. He then states that there is "near universal agreement that radiation-induced cancer occurs in humans based

on atomic bomb data and the protracted epidemiological studies mentioned above." Those studies have limitations as well, but again, his arguments address a different topic. Here he attempts to draw us into a pro vs. con debate of the validity of the LNT model. That's not what the video series was about.

No. False premise. There are doses above which there is little debate about observable effects. Early experiments above those doses have been superseded. Even Cardarelli in his article accepted the LNT above 100 mSv acute dose.

B. Episode 17 (timestamp 6:45 to 10:30) presents several studies by multiple authors that showed a clear J-shaped response for atomic bomb survivors. Beyea failed to acknowledge those studies.

Also, the HPS series was filled with criticisms of the LNT. In the introduction, I quoted some of them from the raw transcripts.

C. Beyea is welcome to ask HPS and others to interview experts to describe their version of the history of the LNT — or do it himself, in which case he can he defend his version of history. The HPS video series has been out for more than 3 y, viewed by more than 20,000 unique IP addresses in more than 700 cities and 70 countries world-wide in the first year of its release! To date, no one has provided scientific evidence that would warrant a significant correction to any episode. We state at the end of each episode that they must provide their sources to support a change. Beyea's work relies on secondary sources and cherry-picked studies that do not invalidate the primary source information provided by Calabrese and presented in the HPS video series.

I cover those supposed J-shaped dose responses for leukemia in Part 2. The number of cases is too small to allow plotting without sampling error bars and calculation of P values, which are high and not supportive of a true J-shape.

That is a sad statement to make. I let readers decide.

False premise. I rely on both primary and secondary sources, primarily on primary sources. I will let readers decide on my invalidation of Calabrese's claims.

25. [p. 511] Beyea states that "selective stakeholder perception" explains why Calabrese's claim of scientific misconduct is in error.

A. Beyea defines this term as "the inability of some advocates to see words in documents that contradict their thesis." In other words, "confirmation bias." Beyea admits to being a victim of it as well. He claims it is not a moral failing and justifies it by coining the phrase "selective perception." He states, "If it occurs repeatedly, an author's work can be one-sided and misleading."

B. Beyea's commentary is replete with one-sided, misleading, and overtly false statements such that it warrants immediate retraction in my opinion. In this context, my recent publication (Cardarelli 2024a) exposed such behavior by leaders within the NCRP and NAS. Beyea "selectively preferred" to rely on NCRP and NAS documents to support his arguments for the LNT model.

If this rebuttal is supposed to document charges of "one-sided, misleading, and overtly false statements," it fails.

False premise. In Question 1, I asked, "Isn't the modern history of the LNT where the debate lies, with modern data, including epidemiological studies, superseding the old data on animals? "

C. Beyea did this in 2016 and 2017 as well — demonstrating his repeated behavior to perpetuate misinformation and make factually incorrect statements. Calabrese responded to his 2016 paper and chose not to respond to his 2017 paper. I note a factually incorrect statement he made in his 2017 paper (see comment 13). Hence, Beyea's

Wow. I perpetuate misinformation? To me, resorting to personal attacks is a sign of a desperation, a sign of the absence of a substantive response.

My 2017 paper, to which Calabrese never responded, is proof that some Calabrese's claims are contested. Cardarelli should have informed viewers of that fact.

False premise again about my sources. Calabrese provided primary information that was often irrelevant because his starting premise of bad behavior was wrong due to overlooking key text in primary documents or making statistical mistakes. At other times, he missed contradictory primary information.

I discussed earlier that the Report does make use of the Neel data.

The British panel went beyond the NAS report in finding an effect in the sex statistics of offspring of irradiated parents. They accepted linear no threshold.

Calabrese's interpretation of the transcript is contradicted by the contents of the report itself. Sources need to be interpreted correctly and checked against the final product. To gain signoff, NAS reports present competing views. Calabrese thinks that Neel's data showed a threshold, whereas Neel did not, writing that it was inconclusive.

I did not say that Calabrese in the video series accused the Committee of scientific misconduct in connection with Neel's early A-bomb genetic data. I challenged the statement of the HPS authors about Calabrese's views, that, "...the 1956 Genetics Committee did not consider Neel's atomic bomb pregnancy termination data, unlike the parallel British committee, suggesting some nefarious behavior or incompetence. " There was no disclaimer in the article by Cardarelli et al. such as given at the end of Episode 11 by Calabrese about his A-bomb data accusation not constituting scientific misconduct but see (Calabrese et al. 2022 at p 7), (Calabrese 2022 at p 12), Video episode 12 after 18:19 minutes, where misconduct over the Neel study is suggested.

The absence of a disclaimer was not Calabrese's fault, it was Cardarelli's. It would have been fairer on my part to Calabrese to have explained that distinction made in the series, and it would have alerted readers to the fact that the HPS authors' synopsis may not have always given a full picture of what was said. Also, Episode 11 said nothing about Neel's praise in his autobiography of the 1956 NAS study nor mention in it of the inclusion of his negative views on calculations using animal data.

True, the two committees were somewhat competitive. The fact that the British could not derive a doubling dose from Neel's data shows that they, like the US committee, found the data inconclusive on that score. The US committee at least estimated a lower limit from the data on Japan.

work reflects the very criticism he's attributing to Calabrese and the HPS — the difference is Calabrese provided primary-sourced information to support his work — Beyea does not.

D. See response comment #4(C) for similar criticisms by other authors of Beyea's publications.

26. [p. 511] Beyea states that Calabrese's conclusion that the 1956 BEAR Panel didn't discuss the Neel Study was "strange on its face…" because Neel was on the Panel.

A. Episode 11 (timestamp 20:15) shows the actual transcript where Muller influenced the panel not to discuss/consider Neel's study (see a more detailed response in response comment #18). Neel shared and discussed his study with the British panel and challenged Muller in a conference at a later date (August 1956). Beyea's paper lacks these details, yet relied on Neel's autobiography published nearly 50 y later (1994) and Neel's 1963 paper to support his claim that it was discussed by the 1955-56 Genetics Panel. Neither of Beyea's references invalidate the language in the 1956 transcript of the actual meeting which was the primary source used by Calabrese.

B. In Episode 11 (timestamp 23:40), Calabrese discusses Neel's 1994 autobiography and acknowledges that Neel didn't mention his work was ignored by the NAS 1956 panel. Calabrese also states (timestamp 27:30) that the panel did not commit scientific misconduct for not discussing Neel's study. He states that the panel was simply biased and may have feared its leaders. Beyea wrote that "Calabrese apparently did not find…" evidence of the committee's consideration of Neels study and justifies his conclusion based on Neel's autobiography. In Episode 11, Calabrese was aware that the Neel's autobiography excluded language that would have supported the fact that the US committee didn't deliberate Neel's study. Calabrese knew it and discussed it in this episode. As a result, Beyea's lack of detailed historical analysis misleads the readers.

C. Episode 15: Follow the Money Trail: "We are just all conspirators here together"provides additional context (and documentation) on why the American committee didn't address the Neel study but the British committee did. At timestamp 4:06, a letter by Neel to Warren Weaver explains why he shared his data with the British panel stating, "I trust you will see it as an attempt at intellectual honesty rather than disloyalty to your committee." Beyea's following paragraphs about doubling-dose is irrelevant here regarding this point.

D. Episode 15 (timestamp 4:56) shows the actual letter from Neel to Beadle stating, "We couldn't prove

that he [Muller] was wrong, but we didn't feel he could prove that he was right. In other words, we felt that there were a number of unvalidated assumptions behind a good many of his points."

Shown on the screen was a sentence from a1959 letter from which Calabrese derives a false perspective on the British committee. Neel did not oppose LNT; he thought the risk was lower than estimated at the time from animal studies. He did not think such studies could determine human risk

27. [p. 512] Beyea provides a quote by Neel stating, "The question is not, 'Is there damage?' but rather, 'Can the damage be detected?'"
    A. The origin of this logic was presented in Episode 18: "Ideology Trumps Science, Precautionary Principle Saves the LNT." Beyea's failure to note this in his paper suggests he didn't view the HPS documentary or has chosen to ignore it.

We clearly saw different things in the video series. Calabrese and Cardarelli think that the lack of a statistical effect is more than inclusive, as Neel and others correctly interpreted his results; Calabrese and Cardarelli think it means threshold. In any case, Episode 18 does not discuss Neel.

28. [p. 512] Beyea writes "Thus, there was no censoring of threshold effects in the atomic bomb data because, as had long been known, there was no suggestion of a threshold in the data."
    A. Episode 11 (timestamp 14:08) shows the transcript where Sonneborn lists the facts they all agree upon before deliberations. The Genetics Panel agreed that "The frequency of genetic mutations induced by any type of ionizing radiation is simply proportional to the total dose delivered." Thus, the reason a threshold was not discussed was because the panel agreed up front that one didn't exist.

The 1956 NAS genetics committee report does not discuss the possibility of threshold for genetic damages. Lack of support for a genetic threshold was discussed repeatedly by committee members as indicated in the available transcripts. It was not rejected based on a precautionary philosophy. Sturtevant, for instance, would frame it as the best bet in his discussions with threshold supporters (at p. 109, 2nd meeting).

    B. Beyea further misleads the reader into believing that "There was no suggestion of a threshold in the data."Episode 17 shows several studies over a 30-y period where a J-shaped dose response is consistently present among the atomic bomb survivors. Further, Neel's original 1955 study did not support the LNT theory. All of this information isn't challenged by Beyea, yet he makes a declarative statement that there was no censoring of threshold effect in the atomic bomb data — when, in fact, he has no basis upon which to make his assertion.

Misleading premise. The Neel study did not reject the LNT theory. As Neel said and I reported, "... it can scarcely be doubted that the survivors of Hiroshima and Nagasaki sustained genetic damage." The study was inconclusive. The sample size, short time, and the limited mutations studied prevented stronger conclusions. Calabrese's claim of a threshold is a fatal flaw in Calabrese's interpretation.

29. [p. 512] Beyea states, "Calabrese and the HPS authors claim that the parallel British (MRC 1956) report reached different conclusions about the atomic bomb data."
    A. This statement is misleading and factually incorrect because it excludes the whole story of what the differences were and why they eventually came to a **similar agreement** about LNT.
    B. In Episode 15 (timestamp 4:15), Calabrese mentions how the two reports were publicized as being in agreement but then explains that it was due mainly due to political concerns because they were trying to speak with one voice. Calabrese then proceeds to describe the subtle differences between the reports — despite their general agreement.

Calabrese claims in the video that the British basically accepted threshold in Neel's data but chose LNT for political reasons. This is hogwash.

    C. In Episode 15 (timestamp 19:40), I state "The American and British reports were published at

Subtle differences? Both reports supported LNT.

The British report was actually less reserved about the A-bomb data, concluding that it supported a radiation-induced difference in sex ratio. The key point is that both committees supported LNT. Calabrese in Episode 15 indicates in response to a question at about 5:30 minutes that the British report found a threshold model, which is not true. I have not seen such a claim in his written works, so I attribute this slip to the difficulty of being forced to work without notes by the organizers on a tight schedule.

Calabrese claims in Episode 15 around 5 minutes. in response to a question, that the British acknowledged threshold in Neel's data but chose LNT for political reasons. This is Hogwash. No threshold in the data.

Hmm. Once again, a contradictory sentence following a quote is left out. Vintage Calabrese: overlooking text. After commenting on the absence of such strong statements in the British report, which are quoted here by Cardarelli, Auerbach wrote: "Yet it is likely that the authors of the British report would agree with these statements." Was Cardarelli shown the full document? She also stated: "It is therefore encouraging to find a considerable measure of agreement between the conclusions of the British and American geneticists."

Misleading quotation. The British report is similar. In the genetics section at pages 63/64, it is stated: "We consider, therefore, that an individual could reasonably accept a total dose to the gonads of not more than 50 r from conception to the age of 30 years...There will be no undue risk to the offspring of parents...." The US genetics committee report recommended a similar standard up to the age 30, but added an additional 50 r restriction for the succeeding 10 years: "That individual persons not receive more than a total accumulated dose to the reproductive cells of 50 roentgens up to age 30 years...and not more than 50 roentgens additional up to age 40... " Was Cardarelli shown this?

The quote cited by Cardarellie from the British report, which also covered cancer. is talking about undue concern, not a threshold, for delayed effects to an individual, not effects to offspring. The British genetics section also discussed undue risk, not zero risk. In any case, a 100 r dose to the age of 40 (US) is not so different from a 200 r dose over a lifetime (British). Thus, Calabrese is wrong to say at about 5:04 in Episode 15 "the acceptable levels that they came up with are profoundly different than what was within the US framework."

The HPS authors shared half the facts. They left off the

the same time but had differing views on the genetic effects associated with radiation exposure. The American report strongly supported a linear no-threshold model, whereas the British report was more reserved." This same statement was in our HPS publication as well. Beyea's statement that Calabrese and the HPS authors claim that these reports "reached different conclusions about the atomic bomb data" is out of context, misleading, and factually incorrect. The video documentary clearly acknowledges the similar conclusions but exposes the difference between the level of agreement between the two reports.

D. In a key reference at the end of Episode 15 titled "The Muller-Neel dispute and the fate of cancer risk assessment," (Calabrese 2020) describes the differences despite similar conclusions. "Despite the pressure to display a high level of international agreement on critical issues of genetic risk, Charlette Auerbach (1956) wrote that 'There is nothing in the British report corresponding to the categorical American statement: 'Any radiation is genetically undesirable,' " or "From the genetic point of view, they (the radiations) are all bad." Likewise, in their remembrance of Himsworth, Black and Gray (1995) stated that the Himsworth-guided British report on radiation genetics "...suggested levels at which an individual not feel 'undue concern about developing any of the delayed effects.'" Letting the report (MRC 1956A) speak for itself, on page 62, item 255 states: "We consider, therefore, that an individual would, without feeling undue concern about developing any of the delayed effects, accept a total dose of 200 r in his life-time, in addition to radiation from the natural background, provided that his dose is distributed over tens of years …. " Beyea's historical analysis missed this discussion that Calabrese discovered.

30. [p. 512] Beyea provides additional context to this statement made in the HPS paper "Today, there remain no observable genetic effects in humans resulting from the radiation exposure studies of atomic bomb survivors." He states this is a form of "selective stakeholder perception" because we excluded content he felt should have been included.

A. Nothing about our statement is incorrect. No declaration was made about a threshold, just that there were no observable genetic effects. Beyea incorrectly inferred that no observable genetic effects was equivalent to a threshold. We didn't use the term threshold — we simply shared the facts as reported by the RERF. Our statement is accurate while Beyea's statement misleads the readers.

31. [p. 512] Beyea writes, "Once again, there was no indication of any dose threshold for genetic effects in the early atomic bomb data."
    A. Episode 17 (timestamp 12:30) shows nine studies in which the data show a J-shaped response for leukemia among the atomic bomb survivors from 1955 through 1978. Although genetic effects are different from leukemia, Calabrese identified multiple studies where a consistent hormetic (J-shaped) response exists among atomic bomb survivors. Beyea fails to acknowledge this or chose to ignore these facts. Instead, he referenced a 2021 study, which he acknowledged provided "weak evidence" supporting his statement.
32. [p. 513] Beyea states that Calabrese and our HPS paper attempted to "undercut" the linearity claims by the 1956 Genetic Panel. This is factually incorrect.
    A. In our HPS paper, we summarize what Calabrese describes in Episode 12: "Was There Scientific Misconduct Among the BEAR Genetics Committee Members?" Beyea misleads the readers by suggesting we had some motive to undercut the 1956 Committee's attempt to reach consensus on the estimates of birth defects from a population dose of 0.1 Gy based on the LNT model. All that is presented in the paper and in Episode 12 are the facts based on the meeting transcript and subsequent publications resulting from their work. Episode 14, "Should the Genetics Panel Science Paper be Retracted," also provides Calabrese's efforts to retract the associated Science paper for scientific misconduct. His request was denied by the NAS President but not because she didn't find evidence of scientific misconduct. Watch Episode 14 for details on why she denied Calabrese's request. Beyea fails to (1) acknowledge the information provided that supported Calabrese's efforts to retract the Science paper, (2) provide primary sources that would invalid Calabrese's documents, and (3) mention Episode 14 in his paper.
    B. The purpose of Beyea's paper was to defend the integrity of historical figures and committees (see response comment #5). He fails to provide evidence to contradict the information presented by Calabrese (e.g., meeting transcript, letters, Science paper). Instead, he attempts to divert attention from those documents by suggesting a motive that "What Calabrese is essentially claiming is that linearity should be dropped because the 1956 Committee's supposedly messed up the slope calculations." Neither Calabrese nor HPS make such a claim or had that motive. HPS used Calabrese's primary sources to understand the historical foundations of how the LNT model came to be.

next sentence which suggested that observable effects should not be expected. Readers of their article were misled. They deserved better.

I deal with the supposed J-shaped leukemia response in early A-bomb data in Part 2, when defending Edward B. Lewis. The data suffer from a scarcity of cases, which is expected at low doses. In the two examples he gives that were available to Lewis for his 1957 paper, there is 1 case that Calabrese accepts in the lowest dose category and 4 to 8 in the controls. P values for threshold I calculated were high, therefore, not supportive of threshold or hormesis. Calabrese does not include error bars and fits data to a smooth curve thereby overfitting the data.

I do not suggest nefarious motives, but Cardarelli protests too much. Both he and Calabrese are stakeholders in the debates over LNT. They have a motive for trying to find fault with the 1956 NAS genetics committee. I have a motive for finding fault with their analysis. Motives are not the issue.

In episode 12, Calabrese says, "I'm reading this, and you know, my memory isn't the greatest in the world sometimes." Not a great recommendation for speaking without notes for 22 sessions totaling 10 hours over a short time period.

I did not discuss this in Part 1, but the NAS president did not need to do an assessment of the1956 committee report to reject Calabrese's desire for a retraction of the reprint of the report in Science. Whatever the outcome of a review, it would not have led to a retraction because (according to Calabrese) the authors could not defend themselves against a third party.

I think that Cardarelli is in a difficult place, given the gaps I have identified in his article about the video series. It is all falling apart. He cannot see the contradictions, especially those that the Science paper itself contradicts. I will let readers decide if I have not provided information contradictory to Calabrese.

Calabrese's charge about the estimates by six geneticists is not about threshold but about the strength of the linear term. The committee did not agree on the strength, but they agreed on no threshold unanimously, from the beginning. It was taken for granted even in the agenda for the first meeting.

Left out of the citing of the NAS code of conduct for NAS members was, "Members should ... promptly move to correct the literature when errors in their own work are detected..."

And: "NAS members shall treat all individuals in the scientific enterprise collegially and with respect..." These are principles of conduct where Calabrese falls short.

I usually am responding to Calabrese's definitions of scientific misconduct, which almost always comes down to falsification of the research record. In general, I define scientific misconduct as ethical misconduct in the scientific enterprise. What is the problem?

A number of Committee members made illustrative calculations of total mutation risk, calculations which were deemed in the report to not be satisfactory to any geneticist, with too many assumptions, with some geneticists thinking they were not meaningful at all, but they were food for thought. ***Uncertainty was not hidden****. Calabrese will not face these disclaimers, which appear in seven parts of the report. I go over in the introduction the mistakes that Calabrese and Cardarelli make by not accounting for the subdivision made in the report between calculations of visible (tangible) 1st generation mutations and total mutations which include mutations that don't show up in the first generation (recessives).*

Hmm. Accusing people of scientific misconduct seems to me to qualify as inflammatory charges. Such charges tend to excite anger and hostility. That's the definition of inflamatory.

I refute this falsification claim in the introduction, as I have done in my 2025 paper and in my 2017 paper. The challenge was for ***total mutations*** *in the first generation, including recessives that show up in later generations for a very long time. Those calculations that did not meet this criterion were properly covered in a different.*

C. Episode 12 (timestamp 17:30) shows the Code of Conduct for NAS Members. It states, "NAS members shall carry out their scientific research with integrity and the highest standards. NAS members shall not commit scientific misconduct, defined as fabrication, falsification, or plagiarism. Scientific error or incorrect interpretation of research data that may occur as part of the scientific process does not constitute scientific misconduct." Beyea fails to even define scientific misconduct in his paper, which was one of its main themes. HPS provided the NAS definition in the documentary. See Episode 12, timestamp 19:20.

D. Beyea claims the 1956 NAS committee "messed up" the slope calculations. No they didn't — each came to the committee with their own calculation; hence, there was nothing to mess up. What resulted from the committee's efforts was unexpected — it wasn't messed up. Therefore, they took steps to falsify and fabricate the research record, which meets the NAS definition of Scientific Misconduct. Their motive was clearly described in the video series.

33. [p. 513] Beyea describes Calabrese accusation of scientific misconduct as "inflammatory charges."

A. Calabrese's statements were taken very seriously by the HPS. As a result, multiple requests were made for primary-sourced documents to support his claim. As President of the HPS, I was satisfied that Calabrese presented his case in a fair and objective manner based on primary-sourced documents (e.g., transcript, letters, Science paper, and exchanges with NAS President). That is why the video series dedicated two Episodes to this topic (Episodes 12 and 14). For Beyea to characterize Calabrese and HPS as making "inflammatory charges" of scientific misconduct without acknowledging the evidence provided in these episodes is sloppy historical analysis and a form of extreme bias. He simply didn't provide an honest or objective assessment on this issue.

34. [p. 513] Beyea quotes from the 1956 report to defend the committee from science misconduct accusations by stating they acknowledged reservations with their estimates and therefore did not commit scientific misconduct. In this context, Beyea states, "The 1956 Committee does not hide the disagreement," and "There was no suppression of the limitations of the calculation, no hiding of their disappointing vagueness nor suppression of disagreements among committee members."

A. Beyea failed to note the falsification of the research record in the Science paper when they said six committee members took up the challenge —

when in fact three committee members refused and three others were arbitrarily removed. See Episode 12 (timestamp 9:20).

B. Beyea failed to note that even after removing three of the lowest estimates in an effort to reduce the overall uncertainty down to 750-fold — the committee arbitrarily decides to just state the uncertainty was 100-fold — without having to provide any justification. See Episode 12 (timestamp 10:10)

C. Beyea failed to note that the committee's fabricated change to the uncertainty estimate was challenged by outside scientists, which ultimately required the NAS President to decide that no such justification was needed. See Episode 12 (timestamp 12:00).

D. See response comment #11 for specifics on where Beyea makes an error in his 2017 paper by relying on secondary sources associated with this topic.

E. Beyea's reliance on the report does not refute the meeting transcript, correspondence between committee members and the NAS president, or the Science publication. Instead, he states that the Committee's disagreements were "judgment calls" thereby supporting his conclusion that they are not scientific misconduct. Again, he never defines scientific misconduct, so the vagueness of his arguments can easily mislead one into believing his historical version, when in fact he is wrong.

35. [p. 513] Beyea states, "Calabrese bases his claim about suppressed differences, not by references to the test of the 1956 report but to selected unpublished correspondence that led him to the following claim, which is repeated in the HPS article" that the Science publication states six took up the challenge when in fact three refused and three others had their estimated removed (falsification of the record).

A. Beyea relies on the 1956 report as if it is the gold standard. It is not. Calabrese based his conclusion on the transcript, committee documents, and letters. These are the primary sources, not the report or associated Science publication. However, the Science article is where the scientific misconduct accusation originated.

B. Anyone can access the "unpublished correspondence" to verify or refute the messaging in the 1956 report. A professional historian would pursue those documents, like Calabrese did. A scientific organization, like the HPS, should require that type of information if it is going to base its reputation and integrity on an "inflammatory" claim of scientific misconduct. Beyea could have obtained these documents too, but he didn't. Instead, his historical analysis resulted in a highly biased and

section of the report. The fact that some geneticists thought such a calculation to not be meaningful was noted. Yet, this is Calabrese's major charge for scientific misconduct, It is based on a misreading of the criterion specified, leading to a false premise.

As I have stated before, the Committee gave a factor of ten up and down for each of the estimates of the six geneticists, which is how a geometrical standard deviation is described. The calculations were generally made from multiplication of five terms. Errors in such products are properly characterized as multiplicative factors, not in terms of arithmetic errors.

The NAS cannot have volunteer committees respond to outside challenges after the reports are published or there would be no end to challenges. Objectors have to publish their own complaints, which will likely generate a response from the authors or a subset. No such letter to the editor appeared in any journal as far as I know in this case.

A statement in the report (reprinted in Science) cannot be made to go away by quotes, selectively chosen or not, from interpretations of a transcript section or correspondence. Calabrese has made a major error by ignoring the total mutation constraint in the challenge calculation.

The 1956 report, also a primary source, is the gold standard for what is written there. The report sets out criteria for the six geneticists, which Calabrese ignores, a prime example of overlooking key historical text. The supposed hidden estimates did not match the criteria for the challenge. They were properly discussed in a different place in the report.

Calabrese presents selective correspondence and sentences from transcripts and uses them in connection with a false premise, namely that the challenge to the geneticists was for any type of estimates, not exclusively for total mutants and with uncertainty ranges. He finds that more than six estimates were submitted but that misses the point. Only six met the criteria. His logic is wrong.

The idea that the 1956 report is not a primary document is wrong, as discussed in the Introduction, using quotes from the American Historical Society.

unreliable paper.

C. The "unpublished correspondence," which effectively negates Section 2.2 of his paper, is described in Episode 12.

36. [p. 513] Beyea states, "What is inexplicable here is how Calabrese and the HPS authors could have overlooked the clearly stated self-critical language in the 1956 report. Anyone serious about assessing their accusations should read the full report."

A. Calabrese's statements presented in his publications and in the HPS document about the science misconduct of the 1956 Committee were based on primary-sourced documents, as clearly detailed in Episode 15. Beyea admittedly relies on secondary sources and keyword searches.

B. Characterizing the HPS effort as not being serious is disingenuous and misleading, especially since Beyea falsely accuses HPS of doing exactly what he did. See response comment #29(D) above as one example of Beyea's sloppy historical analysis.

C. What is inexplicable here is how Beyea overlooked the content in the HPS video series. Anyone serious about assessing his accusations should watch this documentary and then ask how Beyea could have missed the content.

37. [p. 513] Beyea states, "This false theory that disagreements were suppressed can be traced to specific Calabrese publications…." Beyea then quotes Calabrese, who stated that the record was falsified because three of nine estimates were dropped and then the journal Science reported that only six accepted the challenge. The example cited by Beyea has nothing to do with disagreements among panel members. Calabrese specifies where he believed the panel falsified and fabricated the scientific record to support his conclusion that they performed scientific misconduct. There's no suppression associated with these statements.

A. Beyea presents his case stating, "Calabrese is apparently unaware that one of the excluded three committee members (Sewall Wright) did not want his estimate used (Beatty 2006), and the other two did not include all expected mutations in their calculations, so their reports were reported separately in the report."This is a false and misleading statement. Beyea relied on secondary sources to support his statements. Episode 12 (timestamp 2:10) shows Weaver's charge to the 12 geneticists on the panel. The following shows the error of Beyea's historical analysis.

A. Nothing in Beatty 2006 suggests that Sewall Wright did not want his estimate used, therefore Beyea's reliance on this secondary source led to his inaccurate assessment of the facts.

There is no mention in Episode 12 of the criteria set out in the 1956 NAS genetics report for the challenge calculations. Episode 12 is based on a false premise that the challenge was for any type of estimate and did not have to include an uncertainty range.

Calabrese misused his sources as I have discussed several times earlier in this response. Again, I make use of both primary sources from the period, and I reviewed Calabrese's sources. And, by the way, my keyword searches are searches of primary documents.

Cardarelli and I clearly saw different videos. In addition, I criticize what was written in the HPS articles about the video series and in some cases what Calabrese wrote in his articles. Recall that no transcript of the video series was provided, which lets Cardarelli make claims that cannot easily be checked with actual wording. Now, with my own unofficial transcripts of the series in hand, I can hold Cardarelli accountable for what he now claims is in the video series.

Calabrese's logic was wrong, based on false premises, in claiming scientific misconduct on the part of the committee for only including six estimates in the challenge, thereby understating the uncertainty.

*And Weaver's charge to the committee included the restriction to **total mutants** with an accompanying uncertainty range. Why didn't Calabrese note that in Episode 12? It is clearly relevant to deciding on which estimates should be included for the challenge.*

No. Beatty in 2006 writes: "In the end, Wright refused to be party to this particular activity." In the intro, I also presented quotes from Wright's correspondence that support Beatty. Wright submitted an estimate but not for total mutants. He was one of the members acknowledged in the report as not thinking that totals were meaningful.

B. Beatty 2006 states (p. 63) that "In the end, Wright refused to be party to this particular activity." Episode 12 (timestamp 15:10) shows the cover page of nine reports submitted by the panel members, including Wright, who took up the challenge. Beatty was wrong, and therefore Beyea was wrong. Calabrese was right. (Beatty 2006)

*Nine members did not take up the challenge listed in the report to estimate **total mutants**. Sewall Wright did not and would not estimate total mutants because he did not think it was the best way to estimate genetic damage. For instance, he weighted mutations by severity and largely eliminated recessives.*

C. Beatty 2006 (p. 63) also stated that Wright, Neel, and Demerec "...explicity refused to contribute a number to the final report."This is factually incorrect. Neel is the only one of these three that refused to take up this challenge. Sonneborn and Little were the other two who didn't take up the challenge (Episode 12: timestamp 3:54). Wright provided a 9-page summary of his calculations and Demerec provided a 7-page summary of his calculations. Again, Beatty 2006 was wrong, leading Beyea to be wrong.

Wright refused to calculate a number for total mutants because he thought it was the wrong approach, as I wrote in the introduction. He provided a number using his own criteria that left out recessives and weighted tangibles by severity. He also indicated that he belonged with those who thought the challenge not meaningful. Beatty, not Calabrese, was correct about Wright.

A Beyea 2017 discussed Demerec's contribution (p. 372) to the panel's estimate. Yet Beatty stated "…Demerec, explicitly refused to contribute a number to the final report…." Beyea referenced Beatty four times in his 2017 article. Beyea should have recognized this conflicting information, but he didn't. He cannot have it both ways. One of them is wrong — and it is Beatty, yet Beyea references his paper to support his false theory that there was no scientific misconduct with the panel members. (Beatty 2006, 2017). For transparency purposes — the Beatty paper is also included in the references at the end of Episode 12.

No, there was not a "panel's estimate," there were multiple types of estimates. As I discussed in 2017, Demerec's contribution was to the tangible category of mutations, not total mutations. I spent a fair amount of time in 2017 on the distinction in calculations between total mutations and lesser quantities. The distinction will not register with Calabrese and Cardarelli, but it is key to resolving this controversy.

D. Calabrese provides a detailed discussion about the 1956 NAS Panel's activities, which ultimately led Calabrese to conclude there was scientific misconduct. (Calabrese 2015a, b) This was the subject of Episode 12. Beyea didn't provide any primary-source documents to counter Calabrese's conclusions.

Yes, Calabrese provided a detailed discussion, but it was wrong and misleading because he never recognized and discussed the different types of estimates made by the committee members.

E. Beyea's explanation for the omission of the other two is not supported with any documentation — other than his 2017 paper, which also relied on a Ph.D. dissertation by Jolly. Jolly states (p. 348) that, "Nine panel members submitted calculations on the genetic effects of additional radiation exposure." It should be noted that this statement also conflicts with the language in the Science article, which stated that six members provided estimates.

*Once again, nine members submitted calculations, but the six members estimated **total mutations**. The other three made estimate for lesser numbers of mutations or did not include uncertainty range (i.e. Moser).*

B. Beyea implies that Calabrese "suppressed" the panels disagreements based on his publications.

No. It was Calabrese who claimed the committee suppressed disagreements.

This is repetitive, but I repeat that there were different types of estimates submitted. The challenge was for total mutants with uncertainty range. Estimates made by Wright and Demerec were lower, because they did not include all mutations.

Crow states in a 12 March 1956 letter to Weaver: "Among those who attempted a measure of total damage (Beadle, Glass, Russell, Muller, Moser, and Crow), it seems to me that that essentially three different procedures were used. These are not entirely independent, but they do depend to a large extent on different measures so that agreement among them may be regarded as mutually reinforcing."

Although Moser's estimate was similar to others, it was not included with the other six, because of the absence of an accompanying uncertainty range.

It was total damage that the six calculated. Other types of estimates were properly excluded. Calabrese was wrong to want to add in calculations that did not include all damage. By the way, these illustrative calculations have no bearing on threshold.

In 2017, I argued that Demerec's number must have been included with tangibles to explain the low number for tangibles in the report. Regardless, the important point here is that the other two contributions did not belong with the six who estimated total mutations.

*My statement is correct. The report also states, "Concerning these estimates of **total** number of mutants, three things should be said. First, they are clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known." Calabrese and Cardarelli do not tell readers about these strong caveats.*

*The failure of Cardarelli to distinguish between calculations in general and calculations of total mutants is disingenuous. The report correctly states that six geneticists provided calculations for **total mutations**, accompanied by ranges.*

It's unclear what Beyea is referring to, since Calabrese acknowledges throughout the HPS documentary and in his publications that the panel had disagreements. There's no "suppression" of these facts. Episode 12 demonstrates this. The scientific misconduct claim results from the fact that the Panel falsified the research record (e.g., reporting that six took up the challenge when nine actually did) and fabricated the results (e.g., arbitrarily reducing the panel's uncertainty estimate). Beyea misleads the reader into thinking Calabrese "suppressed" the disagreement among Panel members. This is factually incorrect.

38. [pp. 514] Beyea references his 2017 paper stating "Calabrese apparently wanted the committee to combine all the damage estimates regardless of the fact that two of the estimates were for different problem…."This is not true.
    A. Actually, it was Crow who suggested combining the estimates in a letter to Weaver dated 29 March 1956. It is shown in Episode 12 (timestamp 8:14). A more detailed discussion was published describing each of the nine reports (Calabrese 2015). Both the Beatty 2006 and Calabrese 2015 references are listed at the end of Episode 12. It appears that Beyea failed to acknowledge Calabrese 2015.
    B. Beyea further states, "…the committee did not suppress the other two calculations; they contribute to a separate estimate for tangible mutations appears in the report…" Beyea 2017 references the 1956 NAS Report (p. 25) to support this statement. The 1956 NAS Report specifically mentions that six members (p. 27) provided estimates, not nine, and there's no discussion about the "other two calculations" on p. 25 (as Beyea referenced) contributing to the tangible mutations. There is a discussion of tangible mutations, but nowhere does it state that it results from two panel members' calculations; therefore, I was unable to validate Beyea's statement.
39. [p. 514] Beyea continues to suggest that Calabrese's claims about hiding uncertainty are inappropriate (for supporting his scientific misconduct claim) because the 1956 NAS report included disagreements among the panel members' calculations. Here the report specifically refers to six geneticists, not nine. He also states, "If one reads the report section where the calculations are called disturbingly vague and not meaningful to some geneticists, it is hard to see how anyone could consider that the 1956 report hid uncertainty."
    A. The report falsely states that six geneticists provided calculations — when nine did.
    B. Episode 12 (timestamp 8:10), shows a letter from Crow to Weaver stating, "The limits presented on

our estimates of genetic damage are so wide that the reader will, I believe, not have any confidence in them at all." The 1956 report states, "The most probable estimates as thus calculated by the six geneticists do not differ widely" (p. 27). The internal letters conflict with the published report. Calabrese exposed how Crow and the NAS Committee arbitrarily modified the uncertainty estimate, which supports his claim of fabrication of the scientific record.

40. [p. 514] Beyea provides his explanation for the low uncertainty estimate provided by the 1956 BEAR panel by suggesting "that the estimates were best considered as geometric means, not arithmetic means, …" and references his 2017 paper. He criticized Calabrese by stating he "appears not to accept the fact that the estimates were best considered as geometric means, not arithmetic means, which means the uncertainties were geometric standard deviations that appear smaller to the uninitiated." Beyea provides no evidence to support his explanation — except to postulate.
    A. Beyea (2017, p. 363) states Calabrese is not a historian because he didn't publish in historical journals. Beyea also admits he's not a historian but relies on historians when he goes outside the scientific record to rebut trans-science charges like fraud, misconduct, and financial self-interest. He relies on secondary materials prepared by historians like Beatty. He specifically stated, "I particularly want to avoid the approach of postulating the thought processes of historical figures, which can look like an attempt to pressure the reader into a favorable frame of mind. I also want to avoid the risk of missing cultural and historical context."
        A. Beyea did exactly what he claims he particularly tries to avoid. He provided no evidence or documentation to verify that the 1956 panel's arbitrary uncertainty estimate was a geometric standard deviation. Instead, he postulated this scenario in an attempt to discredit Calabrese's conclusion that the uncertainty reported by the Panel was fabricated so the public would be more willing to accept their recommendation (Episode 12, timestamp 10:10). An act defined as scientific misconduct by the NAS Code of Conduct definition (Episode 12, timestamp 17:30).
    B. Using Beyea's "word search" strategy, Jolly's dissertation (p. 332) mentions "geometric" one time in the context of estimating a permissible dose, an unrelated issue. Beatty's 2006 publication doesn't even mention it. (Beatty 2006; Jolly 2002). If these historians didn't explain the low uncertainty esti-

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The concern was about the limits. As Crow wrote about those who estimated total damages: "These are not entirely independent, but they do depend to a large extent on different measures so that agreement among them may be regarded as mutually reinforcing" Thus, the statement about the most probable estimates not differing widely was correct according to Crow. The uncertainty range provided in the report, geometric standard deviations, was still large enough to be disconcerting to readers (a factor of 100). Weaver dealt with that problem by adding caveats, such as, " they are clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known."

The total mutant estimates were a product of 5 terms with uncertainties in each (Calabrese and Selby 2026). Uncertainties in products are handled using geometric means and geometric standard deviations, because the log of the product is a sum of variables. The sum can be expected to be normally distributed. Calabrese and Cardarelli should know this.

The language of geometric means can be found in correspondence of Muller, Wright and Crow. Also, in in Calabrese's latest paper (Calabrese and Selby 2026).

*In discussing the doubling dose presentation, Muller wrote: "„but here I made the mistake of treating it as if it had been an arithmetic mean rather than **a geometric one**." (Weaver 1956) 23 March Muller to Weaver.*

*In discussing the doubling dose, Wright on 18 April 1956 wrote to Weaver "…the preferred estimates should center around the **geometric mean** to minimize the error factor."*

*In discussing total mutants, Crow wrote to Weaver on 12 March 1956 with these words "(assuming Sturtevant's best estimate to be the **geometric mean** of his limits.)*

*Calabrese: "which Crow assumed to be the **geometric mean** of his extreme estimates." (Calabrese and Selby 2026)*

When geometric means are used, geometric standard deviations, not arithmetic standard deviations, are used for uncertainty.

mate, how can Beyea come to his conclusion without postulating? Calabrese's scientific misconduct assertion due to fabricating an uncertainty estimate are supported by letters and other primary sourced documentation. Episode 12 (timestamp 8:10), shows a portion of a 29 March 1956 letter from Crow to Weaver stating, "The limits presented on our estimates of genetic damage are so wide that the reader will, I believe, not have any confidence in them at all." This is further discussed by Calabrese in the episode.

The 100-fold limits presented in the report are also so wide as to startle, I believe, an ordinary reader, but Weaver handled it with dismissive language about the accuracy of the total mutant numbers, while reaching the conclusion that the results were sobering despite the ranges in numerical estimates. "So that the final thing that should be said is ***that in spite of all the difficulties and complications and ranges*** *in numerical estimates, the result is nevertheless very sobering."*

C. Beyea does not provide any calculation to support the 1956 Panel's fabricated uncertainty estimate of 100-fold, yet he attempts to pressure the reader into a favorable frame of mind that the Panel did not fabricate their reported uncertainty estimate. He further missed the cultural and historical context behind the Panel's decision as described by Calabrese, supported by primary sourced documentation, and presented in Episode 12.

Not correct that I have not provided supportive calculations. In 2017, I calculated a 200-fold range from the data I had at the time, which is in the same ballpark.

41. [p. 514] Beyea attempts to dismiss Calabrese's findings of scientific misconduct by stating that neither the committee members' disagreements nor calculations of genetic damage affected any of the NAS Committee's formal recommendations. He supports his argument by quoting Crow "Yet there is a supreme irony. The whole rancorous debate had no effect on the specific recommendations" (Crow 1995).

What? I said that the disagreements did not affect the formal recommendations, which is true. Cardarelli does not respond to my argument but, instead, changes the subject, shifting to the impact of the committee, which has nothing to do with either scientific misconduct or threshold.

A. Beyea misinterpreted the purpose of the rancorous debate. It was a dispute between Muller and Wright over the significance of genetic death, unrelated to the disagreements or calculations mentioned earlier.

B. In the same article, Crow also stated "The National Academy of Sciences at that time had not done this kind of policy-setting report. It has done many since, but this one was outstanding in the way it immediately and permanently changed public policy in radiation protection" p. 424.

These Bullets (C-D) have nothing to do with either scientific misconduct or threshold. I have not challenged Calabrese for his judging the 1956 report on its impact, only his charges of misconduct. Fire away at overselling the dangers.

C. Beyea also fails to include this Crow statement: "In my view, he [Muller] and Pauling, along with others much less visible (including me), **oversold the dangers** and should accept some blame for what now seems, to me at least, to be an irrational emphasis by the general public and some regulatory agencies on low-level radiation in comparison to greater risks" p. 425 (emphasis added).

I have noted in the past that the 1956 report was similar to the British report, as Auerbach agreed. The two reports gave very similar values for the cumulative doses that would be acceptable, as discussed earlier. So, were the British report's authors guilty of something, too?

D. Crow recognized the significance of their 1956 report in setting public policy and admits they oversold the dangers of low-level radiation exposures. Beyea neglected to provide full context of Crow's 1995 paper by failing to acknowledge Crow's statement about overselling the dangers from low-level radiation exposures.

42. [p. 514] Beyea states that "Calabrese overlooked key sentences in a 1947 document that undermine his claim that Delta Uphoff, who later made important contributions to medical research, was a biased investigator."
    A. There is nothing to undermine because Calabrese didn't make this claim - Uphoff and Stern made this statement in their own report (Stern and Uphoff 1947). Beyea even acknowledges that Calabrese said it was Stern who had labeled Uphoff's lab work as biased. Stern (and Uphoff) did so as a potential explanation for the "unexpectedly low" control rate when compared to similar studies. As a result, they concluded "a final interpretation of these results cannot be offered." See Episode 9 "Why the First Human Risk Assessment Was Based on Flawed Fruit-Fly Research,"(timestamps 17:17 & 18:24).
    B. Calabrese provides necessary context on Uphoff in Episode 9 by acknowledging that she was a "very inexperienced, brand new, master's student" when she was brought in to repeat the chronic study of Caspari, which showed a threshold response. (Episode 9: timestamp 15:04) A low control rate, as reported, supports a linear response. A control rate, as estimated by Caspari, would have supported Caspari's threshold finding.
    C. Beyea's comment that Uphoff "later made important contributions to medical research" is irrelevant to his argument.
    D. Calabrese just points outs the unusual fact that they included a statement about investigator bias regarding their own research. Beyea also stated, "To be fair, the actual language in the 1947 report is unusual."
43. [p. 514] Beyea quotes Calabrese's previous publications and states that, "Calabrese has built his accusations that Uphoff was biased on half of a quotation, overlooking the other half." Calabrese observed that Uphoff and Stern wrote, "a personal bias of the experimental" was a potential reason for an abnormally low control rate when compared to other studies. The other half Beyea refers to is, "It is hard to conceive of a bias," which Beyea claims negates Stern's and Uphoff suggestion that they were biased.
    A. Again, Calabrese did not make this accusation. In Episode 9, (beginning timestamp 14:00), Calabrese describes why he questioned Uphoff and Stern's conclusion was uninterpretable and possibly the result of investigator bias as noted by the authors. The quote that Beyea notes where their attempt to dismiss the bias suggestion that "It is

No, Uphoff and Stern did not claim there was bias, quite the opposite. They asked a rhetorical question about whether bias could explain one low batch out of 7, not the total control. They answered the rhetorical question in the negative, "It is unlikely to be the case." Why didn't Calabrese tell viewers about this?

The "unexpectedly low" comment was compared to six other batches in the control set. Calabrese omits this distinction.

A final interpretation was not available in 1947, but it could be made in 1949, when more control samples were available and replication occurred.

Major statistical error. The Caspari study did not show a threshold; it was inconclusive. Statistical non-significance is not a threshold. The results did violate dose-rate independence.

By 1949 there were three control measurements that were low compared to Caspari's. In addition, in my 2017 paper, I found a wide range of values in studies outside the Stern group supporting Stern and Uphoff's 1949 claim that there existed a wide variation in control rates (Beyea 2017 at p 367).

They said that investigator bias was unlikely and explained why. Who raised bias as a possibility? We do not know. But they wanted to dispel any suspicion.

Note that Uphoff had been mentored as an undergraduate by Geneva Sayre (Uphoff 1958). Uphoff only had to follow Caspari's protocol, not undertake independent research. He helped her as I indicated in the introduction.

Not true. Cardarelli does not quote here the response to the rhetorical question of bias, which was, "It is unlikely to be the case." Thus, they said there was unlikely to be investigator bias. How can Calabrese and Cardarelli then claim that Uphoff and Stern said there was investigator bias?

Uphoff and Stern did not say the results were uninterpretable possibly due to bias. This is an untrue statement.

Yes, the "unlikely" sentence is part of a highlighted section shown on the screen for about ten seconds. Calabrese ignores it in his talk as he does in his papers. Why?

Yes, in 1947, Uphoff and Stern had limited examples of control rates. By 1949 they had a number of them. Caspari's was in the minority. The variance of control rates was the explanation for the differences as they state in 1949, after the full replication of Caspari's study. Calabrese refuses to acknowledge that the results were no longer found uninterpretable in 1949.

Calabrese was gentler in 2011, but from the very beginning he has never to my knowledge credited Uphoff and Stern for their statement, "it is unlikely to be the case." And he always glosses over the fact that the bias comment only referred to 1 of 7 batches.

Cardarelli distorts their 1949 paper, which explained that the variance in control rates was large, which they did not know in 1947. No such admission was needed in the 1949 paper, because by then they had found their results interpretable.

Uninterpretable in 1947, but interpretable in 1949.

But what about interpretability in 1949? If they couldn't interpret their results in 1949, Curt Stern, known for his integrity (Beyea 2025 at p 519), would have said so. The evidence Cardarelli accepted was not on point for1949.

I have found no one at the time who ever doubted the reliability of this 1949 paper with Curt Stern as senior author. Caspari didn't. Caspari reviewed a draft and thought the paper should be published as soon as possible (letter to Stern 01/27/1949). As for the more detailed account, Stern put it off to 1950 (letter to Caspari 01/19/1949) and Uphoff was working with Egon Lorenz in 1949 at Argonne (Lorenz et al. 1949). It was never completed, apparently.

Calabrese has accused more than the members of the 1956 NAS genetics committee of scientific misconduct.

unlikely that this is the case" is highlighted in this Episode (timestamp 18:25).

B. In reference to Caspari and Stern's chronic study, which found a threshold effect, Uphoff and Stern suggested it could have been due to an abnormally high control rate. However, they also stated (p. 5), "There exists, however, independent evidence not favorable to this interpretation." This becomes important, as explained by Calabrese in Episode 9, timestamp 22:17 through 24:40. It challenges the validity of Uphoff and Stern's control rate. Beyea failed to incorporate the full context of this discussion in his paper, but it is captured in the HPS video series.

C. Beyea also failed to note that Calabrese stated, "In the final analysis, there was no clear answer whether there was bias to see a lower control to detect a treatment effect, lack of research experience, or simply chance variation" (Calabrese 2011).

44. [p. 515] Beyea claims Calabrese distorts the message that the Uphoff and Stern 1947 report was uninterpretable because no such admission appears in their 1949 Science paper, which relied on the 1947 data. He also stated "The HPS authors apparently accepted his claims on faith."

A. Episode 9 (timestamp 17:19) highlights the following sentence from Uphoff and Stern's 1947 report: "In view of the former results on chronic irradiation, as well as of the fact that the control rate of the present report is unexpectedly low, **a final interpretation of these results cannot be offered**. Several alternative interpretations are discussed" [emphasis added]. Beyea includes this same quote in his paper. (p. 9) The readers can decide for themselves how to interpret this statement, but as the HPS President and producer of the HPS video series, I accepted this statement as evidence that Uphoff and Stern could not interpret their results, which was consistent with Calabrese's interpretation. My decision was not based on faith. Beyea leaped to his biased conclusion which reflects his poor historical analysis.

B. Episode 9 (timestamp 22:00 — 26:07): Calabrese describes the basis for the 1949 Science paper, which was a meta-analysis of the uninterpretable Uphoff and Sterns' studies and two other studies. At timestamp 25:11, the 1949 Science paper states, "A more detailed account of the work will be presented later." To date, no such account has been found.

45. [p. 515] Beyea states, "There is no scientific misconduct here. Calabrese has misread the situation and jumped to inflammatory accusations."

A. Calabrese has consistently argued that the scientific misconduct by the 1956 BEAR Panel was due to falsification of the scientific record by publishing

in their report and in the Science paper that six members took up the challenge to estimate mutations when, in fact, nine did. Also, the panel fabricated the uncertainty so their conclusions would be more likely to be accepted by the public. (See Episode 12.)

B. Readers are referred to response comments #3, #5, #10, #11, #12, #13, #17, #25, and #26 for more evidence and commentary that supports Calabrese's accusation of scientific misconduct.

C. Beyea relied on secondary sources to justify his conclusion that no scientific misconduct was done by the 1956 Genetics Panel. He's misread the situation and lacks primary-sourced evidence to support his version of history. As a result, Beyea's commentary provides misleading and unreliable information.

46. [p. 515] Beyea stated that "Calabrese came to unjustified conclusions, embarking on a decades-long posthumous attack on the character of Curt Stern." He offered complimentary statements by James Neel, Ernst Caspari, and John Lucchesi to defend Stern's reputation and support his attack on Calabrese.

A. Episode 8 (timestamp 7:00): Calabrese compliments Stern by stating "Curt Stern, to his credit, he backed down, just like a real scientist probably should … you showed me the data, the data looks substantial by credible people, and he backed down." This was in reference to his original belief that Caspari's control group was aberrantly high (which led to his threshold conclusion). Caspari challenged Stern's belief and convinced him that his control group was consistent with the literature — and Stern rightfully backed down and accepted Caspari's conclusions.

B. Beyea referenced Calabrese (Calabrese 2015b) in his paper but failed to inform his readers that Calabrese was complimentary of Stern for accepting Caspari's work (p. 434). Beyea came to unjustified conclusions, embarking on a biased attack on the character of Calabrese, which he has done for nearly a decade.

47. [p. 515] Beyea stated that Calabrese accused Stern "of obfuscations and orchestrating coverup…twisted lies to save the LNT single-hit theory." He further states, "This is a relentless barrage of unjustified criticisms, diatribes really, based on a distorted set of false premises and interpretations about Stern and Uphoff that can easily be contradicted by checking the original sources."

A. Beyea's comments are totally without merit. In Episode 8: "Fly in the Ointment" (timestamp 7:30), Dr. Calabrese suggested that there was an effort to,

Calabrese accused Stern of obfuscations and orchestrating coverup, of manipulation or collusion, of deceptions, of scientific deviousness, of twisted lies to save the LNT single-hit theory, of research, editorial, and scientific community manipulations, of bending his ethical standards, and of conspiring with Muller (Beyea 2025 at p 515). I think most people would agree with me that Calabrese is charging him with scientific misconduct.

Cardarelli repeats here the same untruths about my sources, which represent Cardarelli's main attempt at a rebuttal of the contradictions I have presented in Calabrese's material.

I repeat: Calabrese accused Stern of obfuscations and orchestrating coverup, of manipulation or collusion, of deceptions, of scientific deviousness, of twisted lies to save the LNT single-hit theory, of research, editorial, and scientific community manipulations, of bending his ethical standards, and of conspiring with Muller (Beyea 2025 at p 515). Calabrese complimented Stern in one early case, when he could use it to denigrate Muller.

Cardarelli is worried about Calabrese's character, but not the character of researchers like Stern. I don't attack Calabrese's character; I fault his historical research.

What does this have to do with Stern? Stern was not the author of "save the hit theory" according to Calabrese. Furthermore, what does the existence of a motive have to do with saying Stern lied? And that was in 1947, before the replication that "saved the hit theory."

No primary sourced material? I quoted Calabrese's own words about Stern. They speak for themselves.

The video series is not evidence; it is opinion, Calabrese talking off the top of his head without notes.

"...save the hit theory..." exposing a motive of the early researchers. A primary sourced document, a letter, is shown in the video highlighting these words that support Calabrese's statement. Beyea failed to provide a single primary sourced document to support his inflammatory comments about Calabrese. It is Beyea who continues a relentless barrage of unjustified criticisms about Calabrese and now the HPS. His diatribe is a distorted set of false premises and twisted interpretations about Calabrese that were easily contradicted in the HPS video series. This is a spectacular example of his biased historical analysis.

48. [p. 515] Beyea stated, "Unfortunately, Uphoff's contributions to medicine and science are now obscured in search engine results, because articles by Calabrese falsely accusing her of bias, solely or in cooperation with Curt Stern, now dominate her mentions on the web."
    A. This was discussed in my #42 response comment. Beyea makes another false and misleading statement by suggesting Calabrese accused Uphoff of bias. He did not. Stern and Uphoff suggested that a personal bias of the experimenter (Uphoff) could have cause the particularly low control value. Calabrese simply points out this unusual statement in their report. See response comment #42(B) for more detail.

No. They did not say a personal bias of the experimenter could have caused the low control value of the first control batch. They said bias was unlikely and explained why.

49. [p. 516] Beyea claims he has "dug just as deeply as Calabrese into this work (Beyea 2017), including the statistics," which is one of his research areas.
    A. I welcome Beyea's deep research because it contributes to the discussion about the history of the LNT and its validity or failure as a policy. However, his work lacks objectivity, reflects his personal bias, and overly relies on secondary sources. His few publications on the topic don't come close to Calabrese's lifetime achievements in this area. His record is hardly equivalent to Calabrese's. Beyea's publications on this topic could have never passed the scientific rigor I placed on Calabrese to support his interpretation of history. I required primary sources from Calabrese — Beyea doesn't provide any. As President of the HPS, I would not have risked the reputation of our scientific organization on secondary sources.

Objectivity? Calabrese is a leading force against the LNT. He criticizes geneticists only; not those who were promoting threshold. If Cardarelli is promoting Calabrese as objective, he has a lost cause.

Untrue to say that I did not provide primary sources. Cardarelli seems to have a narrow view of what are primary vs. secondary sources. Cardarelli has indeed risked the reputation of the HPS by listening only to Calabrese, only looking at selective documents shown to him, siding with Calabrese over historians, by not getting a second independent source, only putting a disclaimer at the end of credits, and no disclaimer at all in the article, giving the impression to most viewers that the HPS as an organization accepted the views presented.

50. [p. 516; Section 2.4] Beyea states, "Calabrese claims that the National Academies committee that authored the 1972 BEIR report ignored the dose rate dependency of genetic mouse data. That would be incredible, if true, …"
    A. This is another patently false statement by Beyea. Episode 20: "BEIR I Acknowledges Repair but Keeps LNT. Why?"specifically addresses this topic.This is another example of his careless historical

analysis misleading readers on a topic he considered incredibly important.

51. [p. 516; Section 2.4] Relative to Section 2.4 of Beyea's paper, he states, "There is no basis for inferring scientific misconduct."
    A. Calabrese makes no inference that the 1972 BEIR committee committed scientific misconduct. That accusation was directed only to the 1956 NAS BEAR Genetics Panel. Beyea erroneously attempts to conflate the scientific misconduct claim with the 1972 BEIR committee to justify his statement. This is very misleading and questions the reliability of his historical analysis.
52. [p. 516; Section 2.5] Beyea correctly points out that our paper excluded the words "we believe" in regards to Muller's Noble Prize speech (Cardarelli et al. 2023).
    A. Beyea attempts to imply a motive behind this error by suggesting that it was a significant error because it changed the essence of Muller's statement — which was his belief vs. a declarative statement. Beyea does acknowledge that Muller eventually dropped these words two years after his Nobel Prize.
    B. Episode 6 "The Birth of the LNT Single-Hit Theory" timestamp 8:27 and Episode 7 "Pursuit to Be the First to Discover Gene Mutation" timestamp 2:30, includes the entire quote by Muller.
    C. Beyea employs a technique known as the "Logic chopping fallacy" or "nit-picking." This fallacious approach involves concentrating on trivial aspects of an argument rather than addressing its main point or substance. It's an attempt to divert attention from the core argument by highlighting minor flaws or inconsequential details. This technique can create the illusion that the entire argument is flawed or invalid based on minor errors and can derail productive discussions by shifting focus away from the main issues.
    D. It's important to note that while attention to detail is valuable in argumentation, excessive focus on minor points at the expense of the main argument is fallacious. This technique is distinct from legitimate critique and is often recognized as a potential logical fallacy in debates and discussions.
53. [p. 516, Section 2.5] Beyea states that "Muller spoke the truth in stating what he believed. That is not scientific misconduct."
    A. Neither Calabrese nor the HPS said Muller conducted scientific misconduct in his Nobel Prize speech. Although, as presented in Episodes 6 & 7, Calabrese points out several limitations of the Ray-Chaudhuri dissertation that Muller mentioned in his Nobile Prize speech. Further, Calabrese dis-

I should have said that Calabrese's claim was about the committee avoiding and rejecting female mice data, not about ignoring dose rate dependency.

No. He has made such inferences. As I documented in the introduction, Calabrese has stated that ***the "BEIR I misleads the scientific community*** *with regard to BEAR I ignoring human genetic studies." "This seems to be an outstanding deception...." He also claimed that, "This statement is a deliberate* ***misrepresentation of the facts."*** *These are accusations of falsifying the record, i.e., scientific misconduct under Cardarelli's definition.*

I find the dropping of the contradictory part of the sentence to be another example of selective stakeholder perception, not seeing or appreciating contradictory information.

Nit-picking? To point out that missing words change the meaning of a sentence used to brand someone a liar? Hardly a minor flaw. A major one. Inconsequential details? An absolutely essential detail. You can't logically call Muller a liar for making a statement about what he believes, but Calabrese and Cardarelli do.

My main thesis in the 2025 paper was that Calabrese overlooked key historical text. I provided a number of examples, the "we believe" deletion being one of them. Cardarelli argues from a false premise, namely that the deletion was a minor flaw in calling Muller a liar. He avoids the main argument, which is whether or not Muller was a liar in saying that he believed there was no escape from no threshold.

Lying about the scientific record is falsification of the scientific record, which Cardarelli himself deems scientific misconduct. Calabrese has essentially accused Muller of scientific misconduct in his Nobel talk, which Calabrese says was very influential.

Cardarelli thinks that the Caspari study found a threshold, which is a statistical error. The results were inconclusive. This thinking sets up a false premise, one that has led Calabrese to false attacks on both Stern and Muller.

I do not understand what Cardarelli is talking about here.

Cardarelli here avoids the subject of replication

We are to believe Calabrese in calling Mullers paper, "slipshod" and to disregard Crow saying his presentation silenced the skeptics? Calabrese and I are intellectual mites compared to Muller, called 'one of the most creative and influential geneticists of the first third of the 20th century." (Haber 2023 at p 1) Neither of us are capable of rendering judgement on a presentation accepted by his geneticist peers. I thought only physicists could be so arrogant. No methods section? Calabrese almost never has a methods section. Muller and other geneticists didn't use methods sections frequently, and not for presentations at meetings. His References? Not usually part of an oral presentation, especially when presenting the details of a radically new methodology. Albert Einstein's 1905 paper on special relativity had no references (Einstein 1905). A sad moment in the HPS video series was for non-geneticist and non-historian Calabrese to call Muller's key presentation slipshod, and for non-geneticist and non-historian HPS leaders to sit quietly by and accept such non-scientific language.

The video series misled viewers in multiple ways, by Calabrese leaving out and ignoring key contradictory passages from documents, misinterpreting documents, leading to false premises and false charges of misconduct. Also, misleading were several claims of threshold.

covered that Muller knew about the results of the Caspari research showing a threshold response before he gave his famous speech.

B. Again, Beyea is misleading his audience by conflating fact with fiction. He obscures distinction between truth and falsehood, making it difficult for the audience to discern the truth. His failed attempts at a rigorous historical analysis have serious consequences.

54. [p. 517, Section 2.5] Beyea discusses the peer-review process and how it could have been different in 1927 vs. today's standards. He stated "Muller's production of mutations by x rays, for which he won the Nobel Prize in 1946, was replicated and published…within approximately 1 y of his 1927 *Science* publication. He specifically quotes a 1997 paper by Crow and Abraham that Muller "...gave the full details of his work and that the skeptics were silenced."
    A. Episode 4 "Muller: How Ambition Affects Science" timestamp 16:54, shows where Calabrese discusses Muller's paper given at the Fifth International Congress in Berlin. Calabrese states that Muller showed his data during the conference but then proceeds to summarize it as having no methods or references and described it as a "slipshod paper."
55. [p. 517, Section 2.5] Beyea states, "The HPS authors should have let readers know that Muller's biographer has defended him, arguing that Calabrese's 'characterization of Muller and his supporters is unjust, misleading, and hurtful'" (Carlson 2017).
    A. Beyea is relying on a secondary source as a proxy as if it represents truth and is without error. Further, he's suggesting that the HPS failure to inform its viewers was unjust and misleading. The intent of the HPS video series was to inform the scientific community about the historical foundations that underpin the LNT model's use for cancer risk assessment (Cardarelli et al. 2023). It was not to assess the character of Muller; however, Calabrese exposed many scientific and ethical failings of many involved in this story. He based his interpretation on primary documents. The video series did not mislead its viewers by excluding how a biographer took offense to Calabrese's statements about Muller.
56. [p. 517, Section 2.6] Beyea titles this section, "False claim that the 1956 NAS committee engaged in financial misconduct."
    A. The title of this section is misleading and factually incorrect. Neither Calabrese nor HPS made a claim that the committee committed, "financial misconduct." The HPS documentary showsprimary

documentation provided by Calabrese, who summarizes the financial discussions among the committee members. For example, Episode 15 "Follow the Money Trail: 'We Are Just All Conspirators Here Together" presents the concerns about future funding of genetics research. Timestamp 16:50 shows the transcript from the 5-6 February 1956 meeting (transcript p. 35) where Warren Weaver, the committee chair, states, "*There may be some very practical results — and here is the dangerous remark — don't misunderstand me. We are just all conspirators here together. I am not talking as an officer of the Rockefeller Foundation but I will bat my head in the Rockefeller Foundation to try to get a very substantial amount of free support for genetics if at the end of this thing we have a real case for it. I am not talking about a few thousand dollars, gentlemen. I am talking about a substantial amount of flexible and free support of genetics."*

B. Beyea writes "Maxine Singer and Paul Berg attributed Weaver's 'bribe' offer in the second meeting as reflecting Weaver's determination to get a report finished." Here he relies on a biography written by these authors in 2005, some 49 y after the meeting, to support his interpretation of events. Calabrese (and the HPS) relied on the meeting transcript, which does not mention the word "bribe."

C. It's not clear why Beyea would want to suggest a "bribe" was made to the genetics panel, but accusing anyone of committing a "bribe" can be reasonably interpreted as inflammatory and scandalous. Calabrese never used that word in the HPS video series.

D. Presenting the facts is not the same as making a claim of "financial misconduct" as Beyea suggests. This further demonstrates his lack of historical analysis, resulting in him perpetuating misleading and unreliable information.

57. [p. 517, Section 2.6] Beyea states the committee "accepted the linear no-threshold (LNT) model for mutations at the start of their first meeting in 1955 and never wavered. Thus, the idea that Warren Weaver's promise of potential funding … made in 1956 at the second meeting, influenced support for the LNT, is inconsistent with the time."

A. Episode 11, "Creation of the Biological Effects of Atomic Radiation (BEAR) I Committee" timestamp 13:40, Calabrese discusses Tracy Sonneborn's statement during the 5-6 February 1956 meeting (the second meeting; not the first meeting in November 1955). Here Sonneborn presents several statements to the committee in which he tried

Financial misconduct: The implication by Calabrese has always been that the Committee adopted linear no-threshold in order to gain funding. As an example, he has written: "…Warren Weaver, Chair of the Genetics Panel and Director of Research for RF, tried to entice members of the Panel with RF funding if the Panel Report would support RF initiatives (e.g., LNT)." (Calabrese 2018 at p 295).

Calabrese also wrote, "The coupling of funds and outcome was highly manipulative and inappropriate." (Calabrese 2016 at p 9).

In Episode 15, at about 16 minutes in, Calabrese says, "It's all about money, money in these these these radiation geneticists." As support, he then paraphrases Weaver (incorrectly).

I would say that these are all charges of financial misconduct. Cardarelli is wrong to say that Calabrese made no such claim.

Singer and Berg also relied on the transcript, plus their knowledge, no doubt, of NAS committees. They were not members of the 1956 NAS committee. They are very experienced scientists. Berg is another Nobel Prize winner. "Bribe" is their interpretation of the transcript and other information, but I don't think they meant it literally. "Scientific misconduct" is not in the transcript, either but Calabrese infers the term from the transcript and other information.

I stated what Singer and Berg said to make sure I reported all views that I came across, even those that might not be helpful to my interpretation. I don't think they meant it literally. Weaver's remarks were recorded on tape. Singer and Berg knew how hard committee chairs work to get a report out on the street.

Weaver tied his going to bat for funding to making a case for more funding. He tried to do that in a section in the report, presumably to use with RF leadership and others, which had nothing to do with the LNT. His argument was that, "The present state of advance in atomic and nuclear physics on the one hand, and in genetics on the other hand, are seriously out of balance."

to establish the minimum in which the whole committee can agree without dispute. These included (1) that their primary concern is focused on humans, (2) genetic mutations are directly proportional to the total dose (i.e., LNT), and (3) that the overwhelming majority of mutations are harmful. (transcript page 82-89).

B. Beyea claims this discussion occurred in first meeting in November 1955. No discussion occurred that is consistent with his characterization that the committee accepted the LNT and never wavered in the first meeting. It occurred during the second meeting — after Warren Weaver's "dangerous remark" about additional funding (see transcript page 35).

C. Beyea makes two errors by stating (1) that the timing of the Weaver statement occurred after the Committee acceptance of LNT (i.e., Sonneborn's declaration) when it didn't, and (2) that the blind acceptance of the LNT model occurred during the first meeting when it didn't.

Not true. Here Cardarelli is not aware that support for no threshold was taken as a given in the agenda for the first meeting and discussed repeatedly at the start of the first meeting. It is an incorrect use of documents to report only what favors your interpretation. This is another example of how the practice of relying on primary documents is worthless if you don't interpret them correctly. The LNT was accepted before Weaver's remarks about funding.

No. Cardarelli makes an error in assuming that Sonneborn's declaration about the LNT was the first declaration. Acceptance of LNT is so declared in the agenda for the whole proceedings and discussed repeatedly at the start of the first meeting with no objection from any member about the agenda or the support for LNT discussed at the start of the first meeting.

58. [p. 518] Beyea quotes Cardarelli et al. "Personal letters among the NAS BEAR genetics panelists and transcripts of their meetings showed that some members of the radiation genetics community had a vested interest in maintaining this ideology so that they would continue to received research funding." (Cardarelli et al. 2023)

A. I stand by that statement based on the meeting transcripts mentioned above.

Not a good foundation to base the statement of financial vested interest on the misstatements listed above.

59. [p. 518] Beyea asks, "How did past peer reviewers miss the overlooking of historical text identified in this article?" He writes about establishing scientific credibility, acknowledges that peer reviews do not guarantee every part of an article to be true, and that it is assumed in peer review that authors correctly report their findings.

A. In this case, no one is better at performing a peer review of Beyea's commentary than Calabrese. I provide my review based on my extensive experience working with Calabrese in producing and defending the HPS video series. Few, if any, chose to obtain the original documents that refute or invalidate Calabrese's work.

B. Anyone who wishes to independently purchase the original documents used by Calabrese to verify the accuracy and context of his claim of scientific misconduct of the 1956 BEAR genetics panel can do so since all are referenced in Calabrese's publications. Clearly, Beyea chose not to do this. Instead, he relied on secondary sources, digital keyword searches, or evidence of some other subjective

Actually, I have had for a long time all and more of the scientific documents and transcripts that Calabrese tries to tear down and those he relies on. I have cited and quoted from them. I have searched with key words the entire early scientific literature through Google Scholar to find more. I have long had most of the correspondence that Calabrese cites and more, but I did not cite it until now (reluctantly), because I don't think either of us is trained to do so properly. I now show in this document that, once again, he overlooks key text, this time in contradictory correspondence. My most important documents are Calabrese's papers.

Digital key word searches have been of primary documents, namely the early scientific literature, including reports, in Google Scholar.

strategy to justify his misleading and unreliable conclusions.

C. Beyea writes, "Calabrese's charges of scientific misconduct have never been replicated, only criticized." He then writes, "I am aware of only one claim by Calabrese for which someone else … came independently to the same conclusion based on original historical sources." Beyea attempts to dismiss this as an indirect self-citation. I have shown that Beyea relied on secondary sources to support his position, which conflicted with the primary sources from which Calabrese based his claim of scientific misconduct.

It was indeed an indirect self-citation with Kesavan citing to Calabrese as the source of his conclusion (Kesavan 2014 at p 47). Cardarelli doesn't deny it; he changes the subject to his false refrain about my sources.

D. Hopefully, the types of textual errors I have identified here will help future reviewers of Beyea's papers pursue the original documents to justify a claim of what was, or was not, covered in reports, articles, and correspondence. At a minimum, they could start by watching the HPS video series.

60. [p. 519] Beyea writes that, "previous and current leaders of the Health Physics Society (HPS) endorsed a narrative that accuses prominent deceased researchers of scientific misconduct."

The HPS article was a clear endorsement of Calabrese accusations, as was the video series, where many of Calabrese's accusations were repeated by Cardarelli in his summaries at the end of episodes and repeated often at the introduction to the following episode.

A. This is a factually incorrect and misleading statement. At the end of each episode and on the HPS website, the following disclaimer language is provided: "*The views expressed do not necessarily represent official positions of the Health Physics Society.*"

The disclaimer appears at the end of the credits. Few viewers will last that long. The disclaimer should have been expressed by Cardarelli in his ending monologue.

So why wasn't the disclaimer part of Cardarelli's summary at the end of episodes?

B. Beyea failed to acknowledge that this video series was about the history of how LNT came to be the regulatory paradigm and model for cancer risk assessment that is today (Cardarelli et al. 2023). It was not intended to be a discussion about the pros and cons of the LNT model.

C. Also, a consistent message is delivered at the end of each episode stating, "*The HPS is a trusted source of radiation information. We are a scientific organization, and as is offered with the scientific peer review process, viewers are encouraged to send comments or suggested corrections to 'FactCheck@hps.org,' and be sure to include your sources so that we may correct the record if necessary. If any changes are made, they will appear at the end of this episode.*" Beyea did not submit any comments or suggested corrections to FactCheck@hps.org. Instead, he submitted Part 1 of his commentary for publication. He gives no indication of what he plans to include in Part 2. Given the errors and misleading and unreliable information he's provided in his Part 1 commentary, I see no reason to consider his recommendations until he can provide primary sources that challenge or

Without an official transcript, it was impossible to hold Calabrese and Cardarelli to account. Once the article about the video series was published, I had the opportunity to do so.

Cardarelli will not face up to Calabrese compromising the value of his sources by omitting evidence from them that ran counter to his own interpretation.

contradict those documents discovered and presented by Calabrese in the HPS video series.

D. It's worth noting that I was forced to change the original title of my recent HP article because the HP Editors stated that it would not be published unless it was changed (Cardarelli 2024a). I was surprised they allowed Beyea to title his paper as "Part 1" without some hint of what is expected in following papers. It is an odd practice and some could infer it as an intimidation tactic. Frankly, Beyea's paper should be retracted due to his numerous misleading and false statements making it an unreliable source of information.

E. I've included "Part 1" in my title to reflect my anticipation of his Part 2. If there is a Part 2, perhaps the HP editors and reviewers will ensure his submissions are of better scientific and historical quality.

Readers can decide which papers, if any, contain misleading and false statements, Cardarelli's or mine.

61. [p. 519] Beyea states, "They [Calabrese and HPS] wish us to believe that dose threshold models have been rejected for regulation in the past for nefarious reasons, not because they have been found unconvincing or impractical for regulation."

A. There is convincing evidence to support a belief that nefarious activities have occurred in the past to maintain the LNT model. Bill Russell suppressed the results of a cancer and mutation study he conducted in the mid- to late-1950s that showed a threshold effect in mice. It took 35 y before he exposed these results in a publication co-authored with Arthor Upton, Paul Selby, and others to win a court case in the UK in which Russell's colleague, Paul Selby, testified (Cosgrove et al. 1993). Russell explains his reasoning by stating, "It was, therefore, something of a surprise not to obtain a positive result in the experiment described here, and it was feared that publication of a negative finding could mislead the public into a false feeling of safety" (Calabrese, & Selby 2022). Beyea should have cited this in his commentary.

Cardarelli jumps ahead to Bill Russell. I deal with this situation in Part II, although not the 1993 Cosgrove paper that he mentions. I do so here in a preliminary fashion. The paper reports on a small mouse experiment started in 1956 and completed in 1959, which postdates the 1956 NAS genetics study, so is not relevant to it. There were 351 irradiated mice studied and 162 controls.

The paper deals with a study of lifespan shortening. Such studies are difficult experiments and usually have less statistical power than mutation studies due to variability from multiple causes of mortality. The results on neoplasms were contradicted by other studies. The eventual publication of the results in 1993 did not lead to a regulatory shift to a threshold model, so Cardarelli overstates its importance. However, had the results been published earlier the shift in public focus from genetic risks to cancer risks might have happened slightly earlier than it did.

B. In June, 2023, Milloy published an online article exposing nefarious actions taken by prominent people in the radiation community from the EPA, NCRP, and NAS that suppressed relevant scientific information contradicting the scientific validity of the LNT model (Milloy 2023). I published a summary of this article, complete with more than 40 supplementary emails, in *Health Physics Journal* (Cardarelli 2024a).

C. The HPS video series showed evidence of motives to "save" the hit theory (see response comments #9, #10, #47, #52) and financial promises of "free" research money (see response comments #10, #56). I'll let the viewers of the documentary

Recent actions in opposition to the leadership of Cardarelli and colleagues have nothing to do with past regulation or the early scientific period. This is a distraction.

In 1947, the Caspari experiment upset past thinking, which led to the 1949 replication by Uphoff, which everyone at the time, including Caspari, thought did not support a threshold. No one worried about saving the hit theory in 1949. Cardarelli and Calabrese cloud the issue.

decide if those were nefarious in nature or just part of an academic practice.

62. [p. 519] Beyea states, "The misconduct allegations by Calabrese and the HPS authors that are analyzed in this article are not supported by the original text, leading to false premises that have persisted in Calabrese's papers since at least 2014."
    A. This is a misleading and false statement. The HPS made no such allegation of scientific misconduct. In the opening statement of Episode 12 (timestamp 1:00), I stated, "Were their [BEAR Genetics Panel] actions transparent and objective? Was there scientific misconduct? Watch, and decide for yourself." Sharing the history, based on primary sourced documents, does not mean HPS made any such accusation.
    B. In Episode 1, (timestamp 1:50) I describe the purpose of this documentary and state, "*We [HPS] are a scientific organization and as such, seek truth and transparency on how science may or may not be incorporated in radiation protection standards, guidelines, and policies. These videos are offered to the world in a transparent pursuit of truth to garner trust among those who view them."*
    C. Beyea used a clever logical fallacy known as framing bias by selectively emphasizing or omitting aspects of this issue, which distorts the reader's perception and understanding. He artfully exercises his own "selective stakeholder perception" by making this false statement. This leads to other logical fallacies such as the straw man fallacy, where an opponent's argument is distorted to make it easier to refute, or the bandwagon fallacy, where popularity is used as evidence of validity.
63. [p. 519] Beyea writes, "What does a furor at 50 R have to do with the HPS authors' proposal of a dose threshold at 10 R? We are not told."
    A. Again, Beyea missed the point of the HPS video series (and associated publication). It was to present the history of the LNT model, not to validate or refute its use in cancer risk assessment. In response to his question, I refer him and his supporters to read the HPS Position Statement on Radiation Risk (https://hps.org/documents/radiationrisk.pdf).
64. [p. 519] Beyea writes "Calabrese claimed that the committee censored the record in the interest of future financial gain, which it did not. No censoring occurred, as shown by examination of the actual committee report."
    A. This is another factually incorrect statement. I refer the readers to response comments #40 and #56 for a detailed discussion about the financial motive

"No threshold" was accepted by all the geneticists on the 1956 NAS committee, which Calabrese has at times conceded. It was taken as a given in the agenda of the first meeting. Thus, Weaver's talk about future funding did not influence anyone about LNT.

Yes, the HPS authors did make such allegations. I referred not to the HPS, but to the HPS authors (and Calabrese, who clearly made allegations of scientific misconduct). In the article, Cardarelli and coauthors write about learning, "... disturbing details that included several Nobel Prize laureates, the influence of money, scientific integrity issues, ethical and moral dilemmas, and ***scientific misconduct****."*

Also: "This forum article, the video series, and the supporting documentation bring to light the convoluted history of LNT that is replete with ***scientific misconduct*** *and a lack of peer-review."*

Failing to publish a transcript is not being transparent. Single-source journalism and giving only one side of a story is not transparency. Omitting evidence from primary documents is not transparency.

Cardarelli should look in the mirror and remove the beam from his own eye.

I earlier presented quotations from raw video transcripts to demonstrate the anti-LNT bias in the video series and push for change. Here, I mention Cardarelli's editorial remark at the end of Episode 14 about the LNT: "It may be worth reconsidering the issue."

In Episode 22, after listing Calabrese's 7-point plan, he endorses change, saying: "Moving forward, these seven points will help the radiation protection community strengthen future recommendations by relying on a stronger scientific foundation, ***the evolutionary based version of the precautionary principle*** *will be protective for the general public and provide the foundation for regulatory actions based on sound science." There is an agenda here.*

provided to the panel by its Chair. Beyea's summary and conclusion section continues to perpetrate a false and misleading history via his own selective stakeholder perception. HPS presented the source documents provided by Calabrese in the documentary to support his interpretation.

Cardarelli insists in his rebuttal on tying the video series to the HPS, not himself and his team, thereby negating his disclaimer that he put after the credits in the video series. Calabrese omits evidence from documents that run counter to his interpretation.

B. I refer the readers to response comment #39(B), which quotes a letter from Crow to Weaver stating, "The limits presented on our estimates of genetic damage are so wide that the reader will, I believe, not have any confidence in them at all." Calabrese exposed how Crow and the NAS Committee arbitrarily modified the uncertainty estimate, which supports his claim of fabrication of the scientific record.

Cardarelli ignores the fact that Weaver dealt with this uncertainty in the report in multiple places, indicating that even the geneticists found the calculations to be unsatisfactory, with too many assumptions, although sobering. The report states that some geneticists did not think them meaningful at all. Uncertainty in illustrative calculations was not hidden

C. Beyea says he relied on the actual committee report to support his version of events. Calabrese relied on the actual committee meeting transcripts, which conflicted with the report. Calabrese exposed these conflicts and concluded that they met the scientific misconduct definition by falsifying and fabricating the scientific record.

No conflict with final report. Calabrese ignored the language in the report about Japanese data, uncertainty, and total mutants, all of which contradicted his so-called expose based on earlier meeting transcripts.

65. [p. 519] Beyea states, "Furthermore, the early atomic bomb genetic data did not, as Calabrese and the HPS authors claim, show or suggest a threshold dose response."

A. I refer the reader back to response comment #28. Episode 17 shows multiple studies over a 30-y period where a J-shaped dose response is consistently present among the Atomic Bomb survivors with leukemia. Further, Neel's original 1955 study did not support the LNT theory. All of this information isn't challenged by Beyea, yet he makes a declarative statement that there was no censoring of threshold effect in the atomic bomb data — when, in fact, an argument can be made that there was.

Neel's 1950s studies were inconclusive as to genetic effects, as he and coauthor Schull stated. There was not sufficient statistical power. Eventually, Neel did derive an effect, albeit 4-5 times less frequent than predicted from fruit fly data—a doubling dose of about 400 rem. Never threshold.

Calabrese's argument for a J-shaped threshold fails miserably for the A-bomb leukemia data, as I argue in Part 2. Poor P values. Cuttler and Calabrese stand alone, with no statistical support. Calabrese misleadingly plots data points without error bars. Moreover, I have now realized that, for the key dose category, Calabrese redacted 1 of 2 leukemia cases (50%) without alerting readers to this debatable choice. See footnote 5 in Introduction. This was in the first two leukemia studies, those that predated the 1956 genetics committee (which never even discussed cancer) and Edward Lewis's key 1957 paper on leukemia risk.

66. [p. 520] Beyea states, "The HPS leadership has gone along, handing out professional credit, according to the video website, for watching these video interviews of Calabrese that promote falsehoods about historical events and personalities." See response comment #6 for a detailed response. Beyea's comment is inaccurate and inappropriately attacks the credibility of the HPS.

67. [p. 520] Beyea states, "Calabrese and the HPS authors have an important role to play in challenging conventional wisdom about dose response, but when errors creep into their work and remain unresolved for long periods of time, their credibility will suffer. While contrary views are needed in the scientific enterprise to help guard against hardening of the intellectual

arteries,conspiracytheoriesandcharacterassassination (Croc 2011) are not needed."

A. I agree with Beyea's statement that we have an important role to play in challenging conventional wisdom about dose response. However, it is Beyea who's allowed errors to creep into his work by relying on secondary sources and failing to conduct a proper historical analysis. Therefore, his credibility and publications are unreliable and must be treated with great skepticism.

I repeat that I rely on primary sources, particularly those that contradict Calabrese. I also point out how he has omitted evidence from primary documents that run counter to his interpretation. When primary documents are used, they should be used correctly as historians are trained to do, e.g., paying attention to chronology.

B. It is a false allegation that Calabrese and the HPS engaged in conspiracy theories or character assassination. The facts presented are based on primary sources; Beyea's are not.

Not true. I use primary documents to show that Calabrese omits evidence from primary documents that he cites and ignores evidence in documents he criticizes, such as the 1956 NAS genetics report.

68. [p. 520] Beyea states, "No villains need to be created to explain strong disagreements among researchers and within advisory committees."

A. Beyea suggests that Calabrese and HPS created villains in an effort to strengthen the case of scientific misconduct against the 1956 Genetic Panel. That term was never used in the HPS video series. He misleads his readers in this context and exposes his personal bias. He cleverly engaged in a circumstantial ad hominem attack on Calabrese and the HPS by attempting to suggest that the pursuit for truth based on primary sources was biased and therefore false.

The pursuit of truth? Calabrese does opposition research and cannot see contradictory information, leading to false premises on which he bases false charges of misconduct.

69. [p. 520] Beyea states, "The challenge now is to correct the record and prevent such errors from happening in future historical work."

A. Ideally, Beyea should apologize in a thoughtful retraction to his readers and the HPS for falsely attacking the content presented in the video series based on his careless historical analyses and personal bias. He could benefit from hiring someone without a stake in the dose-response debate to check his claims about reports, papers, and correspondence using primary sources instead of relying on secondary sources.

The false secondary source charge is all that Cardarelli seems to have. I would love to have the funds to hire someone neutral to double-check my work. For Part 1 I had one personal reviewer who believes in hormesis. For Part 2, I have been lucky enough to have a hormesis and threshold supporter check many of my sources out of public interest.

70. [p. 520] Beyea states "The HPS authors should: 1. Make clear that they did not check the original documents adequately, which is apparently the case."

A. This is another misleading statement. As the producer of the HPS documentary, I required that original documents support any potentially controversial position expressed by Calabrese. These were shown throughout the HPS video series. I refer the reader to response comment #12 for further insight.

Cardarelli only looked at documents shown to him by Calabrese, a selected set. He was not shown, apparently, those parts of the documents that contradicted Calabrese's interpretation. He was not shown correspondence. E.g., by Sewall Wright, or by Ernst Caspari that contradicted Calabrese's interpretation. Cardarelli had but one source, Calabrese.

71. [p. 520] Beyea states HPS should "2. Update their article and the website of the video series to indicate that questions have been raised about the factual accuracy

and relevance to more recent dose-response assessments of many of Calabrese's historical claims. An errata sheet would be helpful."

A. Please see my response comment #60. In summary, given the errors, misleading and unreliable information that Beyea provided in his Part 1 commentary, I see no reason to consider his recommendations until he can provide primary sources that challenge or contradict those documents discovered and presented by Calabrese in the HPS documentary.

Cardarelli ignores the primary documents that I have used to contradict Calabrese, such as historical reports, scientific articles and transcripts. He seems to think that only correspondence is a primary document, inconsistent with the definition of the American Historical Society. He builds a false premise that I rely on secondary documents for all or most of my critiques. Arguments built on this false premise don't hold. Moreover, he will not confront the charge of omitted evidence based on selective stakeholder perception.

72. [p. 520] Beyea states HPS should "3. Provide references and links to this commentary, to earlier publications, and later, to other properly documented commentaries that they receive in the future that differ from the current website or support it."

A. I refer the readers to response comments #5, #7, #12, #17, #22, #32, and #62. These discuss the purpose of Beyea's commentary (i.e., challenge the accusation of scientific misconduct) and the HPS video series (i.e., describe the historical evolution of the LNT model). Beyea also attempts to conflate his primary purpose by erroneously characterizing the video series as an anti-LNT message.

Claiming that the video series was not an anti-LNT message is perhaps the most incredible claim that Cardarelli has made. This is a prime example of Cardarelli overlooking evidence.

B. During my tenure as HPS President (2021-2023), this issue of providing references that support or refute the validity of the LNT was discussed among the HPS Board of Directors, and they decided not to do it. The challenge would be how to select references and deciding when to end it. There are thousands of publications on this topic. Ultimately, it is left to the researcher/readers/viewers to base their conclusion on the peer-reviewed literature or other reputable sources.

This is hearsay about the HPS Board of Directors. In any case, the issue is not about providing references that support or refute the validity of the LNT. The issue is about the absence of any notification to viewers of the video series or endorsement article by the HPS authors, Cardarelli et al., that Calabrese's views were contested. That omission should be corrected as best it can, by adding links on the website.

Fortunately, readers have my 2025 Part 1 article to indicate the contest.

73. [p. 521] Beyea states HPS should "4. Arrange for corrections, references, and links to be provided to anyone who obtained professional credit for viewing the interviews of Calabrese."

A. I refer the readers to response comments #6 and #66. HPS does not provide professional credits. Beyea should address his concern to the AAHP.

B. As a Certified Health Physicist, I'm a member of AAHP and would suggest they share Beyea's commentary (if it is not retracted) and my responses to it because these exchanges could represent an important contribution to the LNT scientific discussion.

That is a good suggestion for AAHP to share my 2025 paper along with Cardarelli's rebuttal. I would add sharing a link to this response to Cardarelli's rebuttal.

74. [p. 521] Beyea states HPS should, "5. Recommend that the video series be renamed, 'The early history of the LNT by Professor Edward Calabrese.'"

A. I disagree with Beyea. The title accurately reflects the historical foundations of the LNT dose response model for cancer risk assessment. Episode 1 explains why Calabrese was interviewed. No one has come forward with primary sources that challenge or contradict the historical record exposed by Calabrese.

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With all due respect, this is nonsense. Calabrese does not use his primary documents correctly, as I have shown. Because of selective stakeholder perception, he omits evidence that runs counter to his interpretations. He does this in the papers of Uphoff and Stern, Muller's Nobel Prize talk, the 1956 NAS genetics committee report, the transcript of their meetings, and the 1956 British MRC report. These are all primary documents.

And what are Threshold and Hormesis supporters peddling? Not a political narrative?

Here is another example of Cardarelli promoting an anti-LNT agenda and changing the subject from Calabrese's oversights.

DISCUSSION AND CONCLUSION

LNT supporters aren't defending a scientific consensus — they're peddling a political narrative. The LNT model, which claims there's no safe level of radiation exposure, has become a convenient weapon for advancing agendas like overregulation, fear-mongering, and hindering technologies such as nuclear power and nuclear medicine. Critics like Beyea default to politics and logical fallacies because their version ohistory and science is flimsy and highly questionable. Unfortunately, when their tactics of ignoring the facts presented by Calabrese and many others fail, they attack in an effort to discredit and harm their professional career and to silence them into submission. The commentary by Beyea is an example of his attempt to counter the facts presented by Calabrese in the HPS video series on the historical evolution of the linear no-threshold model. The documentary accurately presents historical events based on primary-sourced documents as discovered by Calabrese. Suggested changes are always welcomed, but they must be supported with primary-sourced evidence and not driven by ideology. My criticisms of Beyea are consistent with those from Calabrese, Cuttler, Sacks, and Siegel.

I am trying to get Calabrese and Cardarelli to correct the record on false attacks on the integrity of past geneticists. Cardarelli seems to care nothing about the reputation of past geneticists.

Calabrese does not present facts; he presents his interpretation of historical records, which I have shown have led him to false premises on which he bases his misconduct attacks. Readers can decide.

Sacks, and Siegel eschewed personal attacks on historical figures; they argued in 2017 about the LNT from a scientific perspective at the time. They did not make Calabrese's mistakes. Cuttler is a coauthor of Calabrese's.

The Committee on Publication Ethics (COPE) has established clear criteria for retracting a paper, emphasizing transparency and integrity in the publication process. A paper should be considered for retraction if there is evidence of unreliability. COPE emphasizes that retractions are aimed at correcting the literature and alerting readers to articles that contain such seriously flawed or erroneous content that their findings and conclusions cannot be relied upon. I believe Beyea's commentary meets this standard, and the *Health Physics Journal* should retract it. His commentary demonstrates several critical flaws that undermine its integrity and accuracy, warranting its removal from the scientific record. These flaws fall under several COPE categories, including:

These COPE standards would seem to apply to the HPS video series and the endorsement by Cardarelli et al. in Health Physics. *However, I have never called for their retraction, because regulatory science, like science in general, is contested territory. Disparate views should be out in the open to be contested, rather than left to a separate unchallenged community whose rhetoric can be adopted by political parties to produce wedge issues.*

1. Falsification and Misrepresentation of Data: Beyea's paper misrepresents Edward Calabrese's work and arguments. Instead of directly addressing the primary source data presented by Calabrese, Beyea

After reading this, I am not sure that Cardarelli actually read what I said in my paper.

No. I say that because of selective stakeholder perception, Calabrese omits evidence that runs counter to his interpretations. How hard is this to understand?

Calabrese's claims of misconduct are falsified by primary documents, actually – the sections of them that he overlooks..

Cardarelli loses his credibility by insisting that I do no challenge the "primary-sourced" information presented by Calabrese.

Cardarelli maintains that Calabrese's claims are based on the NAS Code of Conduct. The phrase, code of conduct, does not appear in my digital search of the 60+ Calabrese's papers on history that I have. Nor could I find any such phrase in his papers on Google Scholar. In fact, Calabrese uses scientific misconduct in its commonly understood sense: misconduct by scientists. It is true that in most cases, he claims falsification of the record, but he is vague about many other kinds of conduct, which is not a problem because scientific misconduct is a broad term. The problem is the reliance on false premises set up by omission of contradictory evidence.

Nice try at changing the subject. Are Cardarelli's opinions reliable? Cardarelli's opinions include the idea that the video series are not anti-LNT, that he didn't need to put a disclaimer in his article that would separate Calabrese's views from necessarily reflecting the views of HPS, that he could not be fooled by looking at the documents shown to him by Calabrese, that it was fine not to get a source corroborating Calabrese's views, that a transcript was not necessary to allow Calabrese and himself to be held accountable for their words.

Cardarelli repeats in his conclusions all the false premises that he sets up about my work.

Yes, Calabrese should have been comprehensive in considering the correspondence from Wright and Caspari that I provide in my response to the rebuttals, as well as all the other primary sources I have cited to contradict his interpretations.

characterizes evidence-based claims as mere "theories." This constitutes a falsification of the nature of Calabrese's research and the foundation upon which it rests. As detailed in HPS video series Episode 12, Calabrese's claims are supported by primary source data, which Beyea fails to discredit or even adequately address.

2. Lack of Objectivity and Bias: Beyea's stated purpose is to defend the "integrity of historical figures and committees," indicating a pre-existing bias that compromises the objectivity of his analysis. He relies heavily on secondary sources while neglecting to critically examine or challenge the primary-sourced information presented by Calabrese. This selective approach to evidence is inconsistent with the principles of unbiased scientific inquiry.
3. Misleading Statements and Omission of Important Information: Beyea makes several misleading statements that distort the context of the debate. For example, Beyea neglects to define "scientific misconduct," a crucial omission given that Calabrese's claims are explicitly based on the NAS Code of Conduct. Beyea also inaccurately characterizes "members" receiving professional credit for watching the HPS video series. He fails to recognize that those "members" are Certified Health Physicists in the AAHP, not just members of the HPS. These omissions and misstatements contribute to a distorted and inaccurate portrayal of the facts, violating COPE guidelines on honest and transparent reporting.
4. Concerns about Validity: Given the demonstrated errors of fact, reasoning, and omission of relevant information, serious concerns arise regarding the overall validity of Beyea's conclusions. The paper, in my opinion, does not meet the standards of scholarly rigor and objectivity expected in scientific publications.

In conclusion, Beyea undermined the validity of his defense of historical figures accused of scientific misconduct by (1) relying on secondary sources, (2) mischaracterizing historical facts, (3) using logical fallacies, (4) ignoring evidence provided in the video series, and (5) failing to contradict the primary-sourced evidence presented in the HPS video series describing the historical evolution of the LNT model for cancer risk assessment. Ultimately, his numerous errors and misleading assertions warrant a careful re-evaluation of his claims and consideration for retraction, underscoring the importance of rigorous scrutiny. Comprehensive analysis of primary sources and an impartial perspective are paramount to establishing discourse on a foundation of precise representation.

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Appendix 2. Beyea's reply to Calabrese's rebuttal (in italics)

Version 2. 2 July 2026.

Paper

Confessions of an LNT Heretic: What Happens When You Challenge Biomedical and Environmental Dogma: Rebuttal of Beyea Commentary 2024 (sic)

Edward J. Calabrese[1]

Abstract—This article provides both a personal reflection concerning major professional and scientific challenges that can occur when evaluating the concept of hormesis and the historical foundations of cancer risk assessment/linear non- threshold (LNT) dose-response model and a detailed response to a recent critic.

Beyea: I only assess his historical claims. All non-historical material that Calabrese mentions is distraction, serving to divert attention from allegations that he overlooked key historical text, leading to false premises on which he based accusations of misconduct on the part of historical figures whose dose-response beliefs he wants to discredit and replace in regulation with hormesis or threshold, which he calls optimization.

The assessment briefly captures what happened when an established mid-career scientist (i.e., the author) somehow went "astray" and challenged the central dose- response dogma of the scientific community "establishment" and regulatory agencies. It highlights what can and did happen to me when I was perceived to be a threat to vested interests; and how professional forces became animated, mobilized, and organized to marginalize me and my ideas/research to destroy my reputation and even get me removed from my position of having a tenured university full professorship.

We learn in this rebuttal that Calabrese has many so-called enemies, for which he concedes not the slightest contributory role or responsibility. My interest is getting him to make corrections about his misconduct claims.

This historical background and personal story provide insights on their own but also necessary context when addressing new or recycled criticisms generated out of a mixture of legitimate scientific questions and/or ideological bias. There are also deep and vexing frustrations due to my substantial successes in the hormesis and cancer risk assessment areas.

Calabrese has a role to play in scientific and regulatory debate, but any successes in risk assessment are according to Calabrese and contrarian supporters, contested by others.

This framework provides a necessary backdrop to address recent criticisms of the Health

Physics Society (HPS) documentary, The History of the Linear No-Threshold Model, and my publications on this topic that were the principal foundations for this documentary. In brief, Beyea's evaluation in the Health Physics Journal (HPJ) gives the impression that it is a broad evaluation of my research on the historical foundations of cancer risk assessment. Yet, Beyea addressed only a very limited set of discoveries that were discussed in the documentary. However, there have been numerous significant discoveries (about two dozen) published since the release of the documentary that he failed to acknowledge.

I deal with the mistakes and oversights about misconduct in Calabrese's post-video papers in Part 2. There was only so much room in Part 1. Furthermore, these latter papers belonged in Part 2 because that fit within the statistical-error category that is the subject of Part 2.

All the new discoveries support, greatly enrich/ extend, and do not contradict any aspect of the documentary.

I disagree, arguing in Part 2 that his claims of dose threshold are all invalid, falsifying the idea that dose thresholds in scientific articles could have been suppressed by geneticists.

In addition, the Beyea assessment represents a recycling of information from his previous two highly compromised papers of nearly a decade ago.

Hmm. Calabrese never did reply to my second paper in 2017, which dealt with topics discussed in the video series and the HPJ article endorsing his work. Calabrese recycles his papers in the video, requiring me to repeat some of what I have written before, as well as introduce new information.

These papers, including the present one, display his confirmation bias that is strongly associated with his failure to use primary source materials, a source of novelty and significance in my historical research on cancer risk assessment.

Calabrese is not truthful in saying that I fail to use primary source material. Most important, I found Calabrese's interpretation of primary source materials, particularly correspondence, to be flawed, because he too often overlooked the fact that his cited correspondence was superseded by subsequent primary documents such as final reports and journal articles, which are also primary sourced documents. For instance, Calabrese points to the lament of James Crow in correspondence during the writing of he 1956 NAS genetics committee report. Calabrese correctly reports that Crow was concerned that estimated risk numbers for total mutations would not be believed, but he overlooked how this concern was handled in the final report, namely by embracing the uncertainty, not by hiding it. The report states: "Concerning these estimates of total number of mutants, three things should be said. First, they are clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known "

The overall conclusion was made modest. The split in the committee was acknowledged: "It is, to some geneticists, not a very meaningful way of looking at the problem. To others it adds up to something at least reasonably clear, and in any event very serious."

Failure to use primary sources greatly diminishes the historical and scientific value of the Beyea article, making it susceptible to secondary source opinion misinterpretations, f rank errors, and

bias, as repeatedly shown herein

Calabrese's major rebuttal is his incorrect primary-source defense. He fails to face up to his overlooking of key historical text in primary documents that contradicts his use of selected correspondence. Moreover, I do rely heavily on primary source material. Calabrese, an amateur historian with no review by professional historians, cannot even get the definitions of primary and secondary documents right. Secondary sources in this context are works by science historians who have studied this period, particularly the 1956 NAS committee. Calabrese claims to have found errors in their work, which I rebut in the introduction. Calabrese's second defense is distraction, diverting attention from my allegations.

In addition, Beyea attempts to damage my personal and professional standing/reputation, possibly violating ethical guidelines of the HPJ.

This is frankly hypocritical. Damaging personal and professional standing of historical figures who opposed his dose-response views has been Calabrese's mode of action in his historical work and the way he has gained attention. Moreover, Calabrese, at least, can correct his work and move on. He can still claim that historical figures made errors, as long as he stops overlooking contradictory text due to a poor memory, acknowledged in the video series, or what I call selective stakeholder perception. I do not attack his ethics or character, but he, like everyone else, is accountable for his errors. Calabrese has many talents, but historical objectivity and balance do not seem to be part of the mix.

Thus, the scientific basis for his comments is generally trivial and often devoid of historical foundation and accuracy, while failing to be representative of my body of work over the past two decades.

Readers will have to decide what is trivial and what overturns his logic. Part 2 covers his later work published after the video series in which he continues to denigrate historical figures, relying on false premises generated by overlooking text and making math and statistical mistakes.

The failure of Beyea to use a primary source-based research methodology, especially when such documents are copious, generally available, and essential for historical research on cancer risk assessment, represents a fundamental flaw that should have been addressed and corrected in the peer review process.

The flaw is Calabrese's. We both are amateurs at history of science. I at least have had my work reviewed by a science historian; Calabrese has not. He failed to correctly interpret the documents he used to denigrate historical figures, relying too often on out-of-date information. He does not in his rebuttal face up to this charge. He raises peripheral issues that distract attention.

Failure to do so led to the publication of a historically flawed and unreliable paper and calls into question the fairness of their review process.

In what follows, as part of his distractions, Calabrese attacks everybody, editors, reviewers, historians, regulators, his academic department. At times I felt left out.

Additional comments:

I deal with Calabrese's critiques of additional scientists not covered in Part 1 and his new claims of misconduct in his post video-series papers in Part 2.

His rebuttal is 44 journal pages long, about 44,000 words. Few will read it, but it will say to his supporters that he has rebutted my criticisms, regardless of whether or not he addressed my allegations of overlooked key historical text in primary documents and his making a statistical error with the A-bomb genetic data., A greater number of readers will view his abstract and conclusion, so I have annotated theses sections more fully,

Calabrese's paper has a two-column format. In what follows, I have given each column its own page, with my annotations printed in the new blank column. In addition, to better distinguish my comments, they appear in italics colored light brown.

When my comments are tied to a particular portion of Calabrese's column, I underline that part of his text.

Beyea: As stated above, Calabrese's paper has a two-column format. In what follows, I have given each column its own page, with my annotations printed in the new blank column. In addition, to better distinguish my comments, they appear in italics colored light brown.

For the first 4+ journal pages (8 of the next half-pages), Dr. Calabrese self-promotes and lists many of his so-called "enemies, based on hearsay," taking no responsibility for any enmity that may have arisen over the years. He also complains about members of an NAS committee refusing to cite his radiation work. Based on my assessments, they were wise to do so In any case, I see no need to comment at length until he gets around to my paper, particularly since so much of his writing here involves chemicals and occurred before he entered the radiation field.

Although I am critical of Calabrese in what follows and sometimes annoyed, I recognize that he is doing what he thinks is best for society, as were the early geneticists that he criticizes.

Distraction.

Calabrese: INTRODUCTION

As an introduction to the present paper, I document my history with the concept of hormesis and the historical foundations of cancer risk assessment/linear non-threshold model (LNT) and how I became embroiled in expansive controversies continuing for more than three decades without stop, career threatening actions, and other conflicts. This information will better frame the context of the Beyea (2024) paper and my response to it. I have been a faculty member at the Universities of Illinois/Chicago and Massachusetts/Amherst, Schools of Public Health, Department of Environmental Health Sciences now for over 50 y, with the vast majority at the University of Massachusetts/Amherst (UMass). Over my first nearly 20 y, I developed a well-funded and productive toxicology laboratory and became substantially involved in significant epidemiological studies (e.g., assessing the effects of elevated levels of sodium in drinking water on the blood pressure of elementary school children) (Calabrese & Tuthill 1977; Tuthill & Calabrese 1981).

Distraction.

We also estimated the amount of soil that children ingest (Calabrese et al. 1989; Stanek et al. 2012), with our values becoming the foundation for US EPA exposure estimates that were also applied in most other countries, along with the discovery that there was an increase in miscarriages in those employed in the manufacture of computer chips in so-called clean rooms (Pastides et al. 1988). Our findings received worldwide attention and led to major changes in cleanroom work activities and materials used. My toxicology laboratory research focused on the concept of differential susceptibility/interindividual variability to toxic substances; that is, why some people (i.e., high risk groups) become adversely affected at doses that others apparently don't (Calabrese 1996a, 1996b). This was studied via the use of animal models that mimicked various human disease conditions. In addition, I wrote numerous books from the late 1970s to the mid-1990s on various aspects of high-risk groups and differential susceptibility dealing with factors such as age, sex, diet, and genetics; how to extrapolate animal studies to humans; and how to predict effects from complex mixtures (Calabrese 1978a, 1978b, 1980, 1981, 1983, 1984, 1985, 1986, 1991, 1996a, 1996b; Calabrese & Dorsey 1984; Calabrese & Kenyon 1991).

These efforts led to my being appointed to numerous National Academy of Sciences (NAS) expert committees, the NATO Countries Safe Drinking Water committee, and numerous other similarly high-level advisory committees. I was a member of the NAS panel that voted to end smoking on commercial aircraft in the mid-1980s. I also developed the first and only single exposure carcinogen database over the decades of the 1980s/1990s based on 6,000 published experimental studies (Calabrese & Blain 1999). I was invited several times to share this database and its applications with various NAS Committees, including one addressing acute exposures. This research became a central feature that guided some of their critical recommendations. I was also part of a group of four scientists (which included John Doull, one of the true fathers of toxicology in the US) who were invited by EPA and the American Water Works Research Foundation to undertake a 28-city speaking tour to many leading federal and state environmental/public health organizations, most leading groups in the water works industry, several major research universities, and a number of large conferences related to EPA's then newly proposed rules for risk assessment of volatile organic contaminants. In the course of these and other achievements, UMass accelerated my tenure decision and promoted me to full professor within 6 y instead of the normal 14 y, a clear sign they were more than pleased with my progression.

My life, however, would change in a rather profound way early in 1985 when I received a brochure concerning a conference on radiation hormesis scheduled for August in

Distraction.

Oakland, CA. The brochure described radiation hormesis as a phenomenon that displays biphasic dose responses, with low doses being stimulatory and generally (but not always) protective while higher doses were just the opposite. This description caught my attention as I had seen this biphasic dose response in my first research experience as an undergraduate in 1966 in a plant physiology course, working with a synthetic plant growth inhibitor. I got immediately interested and turned this observation into a research obsession, making it faculty-guided research, and conducting about 40 substantial experiments that showed a biphasic dose response consistently when the plants were grown in soil or hydroponics. We published a paper on this research, never knowing the term hormesis (Calabrese & Howe 1976).

Soon after I received the conference brochure, I called the conference director, Leonard Sagan of the Electric Power Research Institute, to tell him of my earlier plant research that I now thought may have been an hormetic response. He then invited me to develop a paper on chemical hormesis and offered to pay my travel expenses and waive the registration fee to attend/participate in the meeting. This conference paper renewed my interest in biphasic dose responses and the concept called hormesis. I became interested in how significant the concept was, how generalizable it might be, and its public health and medical/therapeutic implications. Furthermore, I was quite interested in how it might impact environmental risk assessment, which was using the linear model for carcinogens and the threshold model for non-carcinogens. I began to wonder whether these models that had been long used in risk assessment had ever been adequately tested and validated, even though I had fully supported their use in my past books, articles, and as a member of various high-level committees. I did not think that by questioning the validity of the linear or threshold response models that I was being anything other than a scientist. How "wrong" I was to ever question what would amount to perhaps the central dogma of many powerful and politically active environmental health scientists and toxicologists in and out of government. It was amazing to see how quickly and brutally the field can act to prevent "deviation." Many of these establishment scientists acted as if they were not free to question beliefs or past judgements about the nature of the dose response in the low-dose zone. It appears that they viewed this issue more ideologically, as a type of job/grant security based on an environmental-based precautionary principle religious tenet. Somehow their training in graduate school to challenge all assumptions was thrown out of the window when it came to the central dose-response dogma of toxicology and risk assessment.

I saw the issues of the nature of the dose response in the low-dose zone and hormesis as critical questions

needing to be addressed and better understood. I sought funding to explore the topic from multiple sources, with initial funding from several federal agencies and a number of private-sector groups, all within the UMass academic framework, governance, and oversight. It was simply one of four distinct lines of externally funded research in which I was actively involved at that time. This is how the academic research game works: external funding governed via university rules/procedures is the key.

Distraction.

During the decade of the 1990s, we constructed a large hormesis database to assess the generalizability of the hormesis concept and the range of its applications. By the end of the 1990s, these efforts led to the receipt of multiple large grants and the beginning of a long series of papers. In 2003, a major event happened when I published a paper in *Nature* that was placed into that journal's publicity package (Calabrese & Baldwin 2003). This led to numerous newspapers, magazines, and other outlets writing stories about me and the hormesis concept. Nearly overnight I became well known, the center of this activity. Even the journal *Science* did a five-page story on hormesis and put my picture in the issue (Kaiser 2003a, 2003b). *US News and World Report* did its own major story, again with a big personal picture (Boyce 2004). The *Wall Street Journal* also did its own major story (Begley 2003). Very quickly the hormesis concept was taking center stage. For the first time, my work on the concept of hormesis was being taken seriously, including what it could mean for biology, medicine, public health, and the EPA and its risk-based actions and regulations. It is interesting to note that while the single exposure carcinogen database was developed in parallel with the hormesis data base, the hormesis concept attracted major funding, whereas the single exposure concept (Calabrese & Blain 1999) did not attract media attention and significant funding. This resulted in me following the academic hormesis funding opportunity, leaving behind the single exposure work after two decades of nurturing.[1]

With this positive publicity came the awakening of a vast opposition to the hormesis concept and me. Hormesis was soon seen by such ideologically oriented scientists more as a threat to the past 30 y of EPA regulation and risk assessment practices than an interesting scientific concept/issue. An organized effort began to oppose many of the articles that I published on hormesis, either by publishing opposing articles or writing letters to the editor. In some cases, the tone and wording were very aggressive, with the beginnings of attempts at character assassination, some of which were caught in time by journal editors to be modified, while others were published as-is.

[1]I have spent considerable time exploring the scientific boundaries of how my hormesis, single exposure carcinogen and my LNT research tracts fit together in a scientifically coherent manner.

Criticizing someone for setting themselves up as sole judge and jury is hypocritical. This is what Calabrese does with the historical geneticists he denigrates, such as Stern, Lewis, and Muller. Setting standards is a Calabrese trademark. Yet, he sees it as grossly unfair when he is the target, but not when he attacks.

Within this context, an effort to create an environmental ethic that had hormesis and me as its target was published (Elliott 2006). The proposed environmental ethical standard would be applied to only one paper published (Calabrese & Baldwin 2003), with the author (Elliott 2006) also designating himself as the sole judge and jury concerning whether our paper passed or failed his ethical standard. Of course, I failed and was guilty as "charged." This grossly unfair and clearly "targeting" paper (Elliott 2006) required a rebuttal (Calabrese 2007), one of a long line of such defenses and explanations that I have made over the course of the past 30 y.

Suddenly, I had become the face of a morally diminished scientist, an academic who had taken a wrong turn in professional life, lost his way, compromised his ethics, and needed to be challenged, put in place, and if that didn't work, then ridiculed or worse. Very quickly I went from being part of the insider establishment and a member of many of the major committees to a person who had to be challenged, attacked, discredited, and marginalized. The opposition was threatened by the hormetic dose-response discoveries and their implications that had been emerging with our papers. I also noted that my submitted manuscripts often received six or more reviewers instead of the normal two to three reviewers, and this happened in multiple journals. It had several effects: while it did lead to many more initial rejections, it also had the ironically positive effect of improving most papers, with all sorts of helpful comments/criticisms that I needed to address, making my subsequent hormesis papers more effective and far better contributions to the field.

This is hearsay, by the way.

An extremely troubling sign of the strategy and tactics of the opposition occurred at an hormesis research group meeting in my office. One of our biostatistics faculty team members, from the Math/Stat Department at UMass, publicly told our group of 14 people that he was quitting our research group because he was told that if he kept working with Calabrese on the hormesis project, he would likely not get tenure the following year. This was a first for me at UMass. I quickly learned that underground ideologues existed and were not afraid to strike fear--and they were good at it. According to this biostatistics professor, the Calabrese research was said to be undermining the past gains to protect the environment and public health and would result in increased risk of harm. The Calabrese research on hormesis was not to be supported or tolerated. So, the young biostatistics assistant professor quit our group, moved on, and did get his tenure. There are many other similar stories of either blatant professional bullying or personal attacks, or the hidden ones that the untenured biostatistics professor shared. I knew for sure now that I had many opponents and some vicious enemies. Some of these enemies were close to home, being

Complaining about blatant professional bullying or personal attacks? This is how Calabrese acts in his papers and in the video.

part of the UMass faculty and possibly even its administration, and they would look for ways to threaten my professional activities/success. If they would go so far as to threaten the future of this untenured assistant professor with two small children, what would they attempt to do to me, given the chance? I knew then that my life would never be easy at UMass or probably at most major academic locations, but the work on hormesis on which I had embarked still remained fascinating to me, and the science was fun. Within the same UMass context of unseen opposition, just a few years ago an article/interview in *The Huffington Post* with a UMass colleague in my building (and my actual next-door neighbor) indicated that over a long period of time he had organized articles and letters-to-the-editor to oppose and challenge many of my hormesis papers, which he thought were simply wrong-headed. He never was one of the co-authors of the challenging publications; he simply remained hidden in the UMass swamp, sticking needles in a voodoo doll and waiting to see my reaction. This story was shared with *The Huffington Post* soon after he had retired and moved. Such is life when challenging the central beliefs (Thacker 2019).

In 2010, as I was recovering from a broken pelvis suffered in a bicycle riding accident, Professor Shrader-Frechette from the University of Notre Dame wrote a blatant attack article about me and my hormesis research, even putting my name in the title in a highly disparaging way (Shrader-Frechette 2010). She would not stop there as she placed my name in the title of two additional papers (Shrader-Frechette 2012a, 2012b). I have never before, or since, seen this done in a professional domain. Shrader-Frechette had previously attacked (but never had placed their names in the titles of her papers) Sir Richard Doll, the discoverer of the smoking and lung cancer association for which he was knighted, and Bruce Ames, of Ames Test fame and a recipient of the US President's National Medal of Science (Smith et al., 2021), for some of their industry related consulting efforts. Now I had made it to that rare level with the likes of Doll and Ames. However, I did them one better by getting my name in the title not once, but three times. It should be noted that the highly flawed and judgmental environmental ethics paper of Elliott (2006) cited earlier was written by one of Shrader-Frechette's former Ph.D. students, showing that the apple did not fall far from the tree. The Shrader-Frechette paper was then shared with my then Vice Chancellor for Research and my then Dean via an anonymous single paragraph letter from someone in the UMass community requesting that I be subjected to a major "trial" for possible ethical and research integrity issues, with the outcome possibly being the loss of my position after some 35 y at UMass in good standing. This is an experience that I don't recommend.

Distraction from his historical research and his claims of scientific misconduct.

He says he has never seen this tactic of putting a target in the title of a paper applied to anyone but him, yet he does it with Hermann Muller, placing Muller's name in a derogatory title multiple times. Such titles as,
"Confirmation that Hermann Muller was dishonest in his Nobel Prize lecture."
"Muller letter reveals scientific scandal that discredits evidence used to support LNT."
"Muller misled the Pugwash Conference on radiation risks."
"The unraveling of a Nobel Prize: How Hermann Muller was awarded the Nobel Prize: A front for eugenics."

Poor memory on Calabrese's part, I guess.

Calabrese does not mention the charges he filed against Shrader-Frechette at her university (Shrader-Frechette 2012). There might be two sides to this story.

After many months of review, evaluation, and meetings with lawyers from the University President's office, I was finally "vindicated" and allowed to survive and thrive. The official records were to be held for 7 y following legal requirements and then destroyed. Several months before the 7-y period ended, I requested a copy of the complete file but was told that it already had been destroyed, apparently violating the 7-y rule.

During this period, I had been exploring the history of the dose response. This eventually led to my doing a deep dive into the life and science of Hermann Muller, the Nobel Prize recipient. I was trying to write a major paper on the history of the dose response, integrating hormesis with LNT and threshold and was told by a friendly critic that I needed to explore the life and scientific accomplishments of Muller in greater depth in order to do justice to this topic.

With this insightful but gentle nudge, my academic focus on Muller began. It was all that simple. I took to heart the suggestion and put all else aside, trying to read everything written by and about Muller, a massive effort. It became obvious that detailed study of Muller was essential and rewarding. These efforts started in 2008 and led me into the world of the history of science and the discipline of professionals in that field, their sources, and their methods. I educated myself to their trade in order to better understand Muller, and it proved invaluable. I discovered that Muller was deceptive/dishonest at his Nobel Prize speech in December 1946, promoting the linear model at the expense of the threshold model for radiation-induced gene mutations. My two papers on this topic that challenged Muller's veracity and character (Calabrese 2011, 2012) generated a new army of highly talented and aggressive opponents, much like the anti-hormesis crowd. It started with the two biographers of Muller, who made it clear that not only was I wrong but that I was mean spirited and unfair, not in Muller's league as a scientist. I was the problem, not Muller. A writer from The Netherlands interviewed me in depth on the Muller Nobel Prize story and had a contract to have his article published in *Science*. When he sent it to the *Science* editor (I have a copy), the editor forced him to totally rewrite it, with me no longer being the interesting and groundbreaking professor but one who had lost his way and his ethics (Crok 2011). All this happened while my fate was being decided by the UMass administration.

An example of ideological hegemony may be seen in the following bizarre vignette. In December 2017, I was invited via a telephone call and a follow-up confirming e-mail to give a Plenary talk for a Special Session sponsored by the American Academy of Health Physics (AAHP) at the forthcoming 2018 annual meeting of the Health Physics Society (HPS) in Cleveland, OH. All my

He educated himself in the history-of-science trade? Certified himself it seems. Must have missed a lot, such as the definition of primary and secondary documents.

He takes no responsibility for negative reactions to his attacks. He is prone to inflammatory remarks that produce anger and hostility. He will take the slightest hint of wrongdoing to claim with certainty that he is right, others are wrong and they are complicit in challenging him, all because he overlooks contradictory text. In this rebuttal he ignores all the examples I give of his overlooking key text, including failure to note the use of the A-bomb genetic data stated in the 1956 report, the "total mutants" restriction on the challenge accepted by six geneticists, and the acceptance by the committee members of LNT long before any promise from the chairman. Calabrese has no response to these oversights, only attempts at distraction. Yes, he is a problem for a balanced accounting of the past.

travel, lodging, and registration fees would be waived. I was excited because this was a clear sign that my papers on the historical foundations of cancer risk assessment were resonating throughout the radiation community. Furthermore, giving an invited Plenary presentation by one who was not a member of the AAHP or HPS was a special honor and privilege. However, about 6 wk later, my excitement came to an abrupt end. I received a telephone call from the person who invited me, now disinviting me. He told me he was sorry, but there was an administrative change or something. The bottom line was that I was now out. There would be no Plenary talk for me, no all-paid trip to the conference. I did not believe the explanation since I had directed too many conferences and had invited too many Plenary speakers. The simple truth is that Plenary speakers almost never get disinvited. I was far too experienced to fall for that story. In fact, the situation is just the opposite. The conference organizers are so pleased when the Plenary speakers accept. I then spoke to his boss and then to his boss's boss, but all gave the same answer. I was told that I was out and not to call back again.

As bad as this story was, I wanted to know the truth, and 4 y later the truth emerged unexpectedly from a Freedom of Information request by me. I obtained emails of prominent members of the HPS, who just happened to be leaders in the Centers for Disease Control (CDC), the National Committee for Radiation Protection (NCRP), and EPA. I learned that three AAHP/HPS leaders somehow realized what a "danger" I was to their society and the world, and they made up the flimsy administrative glitch excuse. They also got one of their members (i.e., John Boice, the long-standing president of the NCRP) to replace me. Now John would be the Plenary speaker, not me. This act was simply another in a long line of professional decisions/actions to marginalize me and my research, a job undertaken by a self-appointed AAHP/HPS group. It was effective, bloodless, and perfectly seamless. Only I knew I had been taken down in a professional sense. Yet the sad truth behind this story was reported by Cardarelli (2024) based on my obtaining the primary documents, the now damning emails of the group. The group was successful, but now they had been exposed. The major point of this tale is that I have been the target of powerful yet very talented and beloved members of the radiation community who also know how and are willing to take down a person who is comparably achieved but who thinks differently. They make sure that their side wins, whatever means it takes. It is how they play the game, and few know about it or understand it.

I was under the microscope and had become public enemy #1 to four groups that worked in different ways and who were plotting their next series of challenges. There

More distraction.

Again he takes no responsibility for this, with his personal attacks on scientists. He says about Curt Stern: accusing him "of obfuscations and orchestrating coverup, of manipulation or collusion, of deceptions, of scientific deviousness, of twisted lies to save the LNT single-hit theory, of research, editorial, and scientific community manipulations, of bending his ethical standards, and of conspiring with Muller (Beyea 2025 at p. 515).

Calabrese says about the members of the 1956 NAS genetics committee at about 16 minutes into Episode 15, "It's all about money, money in these these these radiation geneticists." He has no basis for this incredible inflammatory charge.

Willing to take down a person who is comparably achieved but who thinks differently? This is what Calabrese does to his historical target who supported no threshold.

Calabrese accuses Muller of "letting ends justify the means, of data obfuscation, of selective interpretation (Calabrese and O'Connor 2014 at p. 464); of deceit (Calabrese 2015 at p. 433); of twist(ing) the truth, of direct lies, of data manipulation, of censorship, of misleading behavior (Calabrese 2017a), of Intentional non-veracity and of lack of intellectual honesty (Calabrese et al. 2025 at p. 13)". Beyea, 2026 submitted at p. 22.

Transforming the history of cancer risk assessment? I do not agree.

Here, Calabrese promotes himself. He can point to no historian who supports his theories. I have reviewed his supposed historical interpretations and found then to be false as related to scientific misconduct due to false historical and scientific false premises. Readers must decide. Again, he takes no responsibility for his personal attacks and supposed moral superiority that many have found offensive.

This refusal to cite Calabrese's LNT publications shows the low regard in which they are held by NAS committee members. He really stands against the scientific world with his beyond-science approach to taking down LNT by denigrating early radiation geneticists.

Unfortunately, Calabrese has not become proficient at history of science as I have found. He knows not even the definition of primary and secondary sources. His false statement about my sources is Calabrese's defense, along with distraction. He cannot respond to the overlooking of key text, for instance in the 1956 NAS report, which is a primary document that he mischaracterizes.

was the anti-hormesis group, the LNT supporters, and the field of radiation genetics that Muller created, nurtured, and matured. I had attacked their beloved leader and hero. I also knew that there was a silent and powerful group at my institution that was watching me, ready to act in any way they could. Thus, my life had become one of constant attacks, debate, and large-scale opposition. I also had become targeted by many now former friends, some of whom had been close friends, who were now part of the opposing forces.

Yet, I continued to be highly motivated and productive in both the hormesis and history of cancer risk assessment domains, transforming these fields. As for hormesis, in the 1980s the terms hormesis or hormetic were cited only about a dozen times per year in the Web of Science database. Now it approaches/exceeds 20,000 per year, quite the growth and achievement. That is, despite the opposition, threats, and attempted professional and personal embarrassments and humiliations and loss of close "friends," this idea prospered and has been adopted by many scientists worldwide who use it to transform and advance their own research agenda and enhance human health practices. In the case of LNT, my numerous new discoveries have transformed the entire historical foundation of cancer risk assessment, turning it upside down, exposing mistakes, corruption, and bureaucratic malpractice at EPA and throughout the world. My name has long been mud with those groups that oppose what I have done on hormesis and LNT. The most recent NAS radiation committee voted to not even use the term hormesis nor to cite my LNT publications (Cardarelli et al. 2023).

This lead-in sets the stage for the next focused attack, this time by Beyea (2024) in the *HPJ*. Beyea has tried this before (Beyea 2016, 2017) and was shown to be incorrect on many issues as far as I am concerned, somehow missing obvious and well-documented findings and failing to approach the research with the most appropriate methods of the history of science. In fact, these failings made communicating with him via letter to the editor nearly pointless. Furthermore, LNT history is highly dependent on primary sources, that being the world of the professional historian of science, something that I have tried to become proficient at over the past 15 y. In the case of Beyea, he still lives fulltime in the world of secondary sources that are subject to the biases and errors of others and repeatedly so. His research is largely embedded within confirmation bias. He simply does not make the necessary efforts to do the job accurately and fairly. Yet, the editors of the *HPJ* failed to understand this major point, selecting reviewers who apparently had little, if any, understanding of the role of primary vs. secondary sources, and they published his manuscript, which does not even remotely satisfy academic standards (Beyea 2024). Such actions do little to

inspire confidence in the peer review process, when the editor and reviewers apparently fail to adequately appreciate the nature of the methods used in history of science research.

This prologue was intended to show that my activities in the hormesis and LNT/cancer risk assessment domains have been highly successful. They have not only withstood the test of time and numerous critics but have also attracted large numbers of scientists to the hormetic dose-response concept, its mechanistic research, and its vast biomedical, public health, and therapeutic applications. The LNT story has been especially challenging to the regulatory and research communities because it shows that the entire field of cancer risk assessment was corrupted from the start by revered leaders, including Muller and many others (Calabrese 2015a, 2015b, 2019a, 2019b, 2024). As noted, I was once one of Muller's admirers as well, but I was able to set that aside and independently evaluate his field, now having essentially rewritten it. It is understandably embarrassing for the "establishment" and regulatory agencies to be told that they have long been wrong, misleading the public when they have been shown exactly where the errors occurred. This is the story of my LNT effort in a nutshell. However, it has created considerable hostility and with its successes has generated much frustration in some who oppose what I reported. I will now summarize my disagreements specifically with the Beyea (2024) paper.

There is no citation to any methods used in the history of science. Calabrese regularly makes his own standards to suit his purposes. He does not follow the AHA. See footnote 11 in the introduction.

Yes, Calabrese promotes his LNT assessment as being successful. I have challenged that assessment in connection with his scientific misconduct claims and his historical claims of threshold (Part 2). He avoids my charges of overlooking key historical text, repeating the same stories and self-promotion as a supposed answer, With my work, readers can now hear the evidence that is absent from his claims of a corrupted field..

Calabrese cannot help claiming that his work is true. He cannot see his work as an interpretation of documents that is contested. He has no support of historians of science for his theories. As is his practice, when an expert like Jolly disagrees with him, denigration follows, as in political campaigns. Calabrese repeatedly describes Jolly's work with the inflammatory words, flawed and compromised.

THE BEYEA PAPER

The Beyea (2024) paper purports to be a criticism of the activities of the HPS committee that created a 22-episode documentary on the historical foundations of cancer risk assessment. I was first contacted in July 2021 about giving a taped interview of indeterminant length but probably on the order of 2-3 h. The interview was taken in late October 2021 and lasted over two consecutive days, about 14 h in duration. It was placed on the HPS website in April 2022 after editing and insertion of numerous required historical documents using mostly primary sources that provided documentation for my comments. The HPS committee wrote a subsequent paper summarizing the documentary that was published in the *HPJ* (Cardarelli et al. 2023). Thus, the paper by Beyea reflects an activity now over 3 y old. While the paper of Beyea is highly critical of the actions of the HPS committee, including the documentary and their paper, he mostly directed his focus on some of the areas over which I was questioned and my related publications. The following narrative documents numerous errors/omissions in the Beyea paper, showing his paper to be highly

Calabrese again makes up his own standards, this time for disclosure, and tries to act as legislator, judge and jury.

Repeating challenged work is not a response, it is a distraction from my charges. His rebuttal is so long that few will read it.

His moral outrage, rather than an answer to overlooked text and statistical error, reflects the absence of a substantive response.

He shifts focus in all directions away from the charges, blaming not just the author (me), but peer reviewers and the editor. All are against him No blame accepted.

Over the top technique, indicative of no substantive response.

Now he goes for retraction, another distraction from my charges of overlooking key historical text.

Everyone is unfairly against him and the stories he tells.

Massive system failure? He doth protest too much.

He makes here his own definition of primary documents, which do not match those of AHA. And he leaves out my use of transcripts and some correspondence.

unreliable in that it failed to disclose conflicts that could bias the peer review, requiring retraction.

The following evaluation and criticism of the Beyea paper is massive, highly detailed and extraordinary, something no editor or publisher wants for their journal. This level of effort was necessary to demonstrate for the reader how biased, inaccurate, unacceptable, and unreliable the Beyea paper is. In over 50 y of academic peer reviewing and having been engaged in numerous past professional debates and disputes, I have seen nothing that even remotely compares in the lack of quality of the published paper and its associated peer review and editorial judgments. Over two dozen areas have been highlighted and documented in detail, most of which contain multiple significant research errors and failed professional judgments. The detailed criticism is necessary not only to correct the mass of mistakes and unacceptable scholarship by Beyea but to document poor editorial judgments of the *HPJ*. This paper represents the so-called perfect storm, which combines the poor scholarship and bias of the author with the lack of knowledge and perspective of the peer reviewers and poor judgement on the editor's part. The paper is so riddled with mistakes, fundamental misunderstandings, and bias that it is far beyond being even a highly unreliable paper. The Beyea paper represents the "poster-child" for what a retracted paper would look like based on the retraction criteria of the Committee of Publication Ethics (COPE). The extent of unreliable information is massive and has been documented in considerable detail. There is not an area that is addressed by Beyea that does not contain unreliable information. His paper seems to fit perfectly the definition of what is unreliable, and this fits the COPE definition of what should be retracted. Since the *HPJ* follows the guidance of COPE in such matters, the Beyea paper needs to be retracted based on the information provided herein. In addition, a similar and equally massive criticism of the Beyea paper is provided in the companion paper of Cardarelli (2025), which details a long string of similar flaws that specifically relate to the HPS documentary, showing numerous striking mistakes and other professional failings of Beyea and exposing again the deficiency of the peer review process and editorial actions. That such a failed paper by Beyea could have passed a peer review at the *HPJ* calls into question the judgement of the editors of this journal. A reading of the Cardarelli paper and the present one will demonstrate a massive system failure, from Beyea to the journal leadership. Let the story begin.

Beyea fails to use primary source documentation

Beyea's analysis is entirely based on what is present in the public domain within the realm of journal publications, dissertations, NAS activities/reports, and government reports. The unique methodology and assess-

ment of my research makes extensive use of preserved private (i.e., primary) documents such as correspondence, research notebooks (like Muller's Nobel Prize research notebook) and other unique historical materials, which I shared with the HPS staff as required during the devel- opment of the documentary. The Beyea paper reflects a failure to seek the most accurate and fundamental infor- mation. By failing to obtain primary documents, the Beyea strategy for evaluation and communication leads to a mistaken impression among the journal and readership by providing a false impression of professionalism. Beyea (2017) has even acknowledged in earlier writings that he failed to use or even try to obtain primary documents. Yet, he still continues along a path that is easier, but far less historically and scientifically reliable. As long as journals such as *HPJ* permit this, they participate in this flawed process.

The disagreements that occur on the present topic of my research are largely in the realm of the history of science. Professional assessments by those researching in this domain require and demand the use of primary sources to the extent possible/reasonable to avoid the bias and potential errors of various secondary sources. For example, Beyea (2017) tried to characterize mutation risk estimates of the BEAR I Genetics Panel (1956) based, as he writes, "solely on secondary sources by historians," leading him to offer unverified speculative estimates when the primary documents were available. Such reliance can also lead to missing essential papers in the scientific literature that are beyond the technical education of most historians. Thus, the approach of Beyea placed his research in the form of double jeopardy as will be demonstrated herein. Thus, Beyea's lack of proper research can lead to serious mistakes and failure to detect Panel bias/deceptions and mistakes, missing fundamental findings as will be discussed in more detail later.

Beyea (2016) also made a serious error in his evaluation of the BEAR I Genetics Panel, relying on the memory of an aged Bentley Glass concerning how massive radiation risk assessment disagreements were handled by the Panel. If Beyea had read the meeting transcripts (which he claims he had) and letter exchanges of the Panel (which he makes no effort to discover or obtain), Glass's failed memory would have been obvious and the basis of his serious error. This error of Beyea compounded the problem, resulting in yet another serious error concerning Panel scientific misconduct activities that my research exposed and documented with primary source materials. Such mistakes of Beyea were emphasized in my rebuttal to his earlier paper (Calabrese 2016). As will be shown later with the case of James Neel, Beyea failed to learn from these past mistakes, only to repeat the process again.

Here Calabrese takes material out of context, as I showed in the Introduction. He refers to a sentence that dealt with one instance, where it was not necessary to go to primary documents to prove a point.

I have shown in the introduction how Calabrese here misstates what I wrote, misleading the reader with his "solely" partial quotation of mine..

As I argue in Part 2, Calabrese has his own problems with technical education, particularly in the area of statistics. Nobel Prize winner Paul Berg and Maxine Singer reviewed transcripts and correspondence about the 1956 Genetics committee. Their technical capabilities exceed Calabrese's; yet they found no scientific misconduct. Moreover, I show that the historians Beatty and Jolly were correct in this exercise and Calabrese was wrong.

My 2016 papers are a distraction. Calabrese refuses to address the actual language used in the report about how the disagreements within the 1956 Committee were handled. Calabrese also refuses to face the restricted nature of the challenge to which Glass was responding. Calabrese insists on adding in estimates that did not meet the bounds of the challenge. His premises are wrong.

These private documents are in the public domain and can be obtained from libraries, just as can the NAS transcripts. In 2017 I obtained correspondence from the same libraries and collection used by Calabrese. I often comment on correspondence described in papers, including by Calabrese.

He says unique methodology. What happened to the methods of science historians?

(Continued)

I say that he omits information, leading to false premises about documents. No defense is made. I say that he misinterprets his information. He does not respond.

If you build on false premises, you end up with false conclusions. The false premises come from documents he cites. His definition of primary documents appears to be his own.

Interpretation is key. Calabrese, historically untrained, makes mistakes in interpretation as I repeatedly have shown.

He is completely misleading about my statement about secondary documents. He omits key parts of the sentence, just as he did with Muller, changing the meaning. I did not say I relied solely on secondary sources. I find this misstatement by Calabrese to be outrageous. I said I did not need to go further than secondary documents in one case because it was unnecessary to do so.

> *"Based solely on secondary sources written by historians, I have not been able to figure out exactly which committee members' calculations went into generating the 50,000 tangible mutation estimate, but it is clear that Demerec's low estimate must have been one of them." (Beyea 2017 at p. 372).*

I have identified articles using searches of Google Scholar that Calabrese missed.

What does this have to do with Calabrese missing the total-mutant distinction in the report? Nothing. Moreover, Calabrese is wrong. He missed correspondence, as I show in the Introduction.

Again, Calabrese tries to set standards outside his toxicology field.

He tries to define primary sources as private correspondence, not as all the documents of the day.

No mention here of my 2017 paper, only 2016, to which Calabrese never responded, nor my 2025 additions. Cuttler is a hormesis supporter and has published 33 articles, mostly about hormesis, in the journal of which Calabrese was editor, Dose-Response. Cuttler has also become a coauthor of Calabrese's. Cuttler has no independent knowledge of the history of the period, so he is just channeling Calabrese. Cuttler is responding here to a critical letter (Beyea 2016b) that I wrote about one of his publications in HPJ (Cuttler 2016).

Cuttler is rebutting my 2016 paper. What happened to 2017, which included my replies to critiques by both of them? Why no rebuttal from either Cuttler or Calabrese to my 2017 paper?

The net result is a newly published paper (Beyea 2024) that once again does not meet acceptable professional standards. This error reflected not only the failure to use primary sources, but it allowed Beyea to demonstrate his confirmation bias (i.e., that the BEAR I Genetics Panel did not commit scientific misconduct when the evidence based on primary sources indicates that it did), making a mistake that would support his bias and misinform readers.

The Beyea (2016) criticism of Calabrese (2015a) was commented upon by Cuttler (Cuttler, 2016). The statement below is quoted from the Cuttler (2016) paper, providing a unique insight into the quality of the Beyea paper and his professional scholarship:

"Beyea attempts to discredit the very serious charges against the NAS of the 1950s by Calabrese (2015) without delivering specific challenges about any of the official transcripts of the meetings and the many signed items of correspondence that Calabrese listed, and does not challenge any of the analysis and conclusions. Instead, Beyea states that no one should believe what Calabrese has written until they read the rebuttals of Cicerone & Crowley (2014) and Crowley et al. (2015). Beyea omits mentioning that this Cicerone and Crowley response did not address the factual basis of Calabrese's numerous assertions; it merely tries to defend the reputation of the NAS, led by Cicerone. Nor does he mention the Calabrese rebuttal (Calabrese 2014). Beyea does not clarify that the Crowley et al. rebuttal is totally unrelated to the Calabrese historical assessment; it only defends the BEIR VII report. Thus, this statement by Beyea is misleading and implying that there was factual criticism of the Calabrese historical assessment when there was none.

There are no theories; the stated activities and decisions of the 1950s actually happened. Beyea's "most detailed" letter (Beyea 2016), which offered a series of alternative interpretations, has elucidated a 13,000-word rebuttal from Calabrese (2016). In his rebuttal, significant newly uncovered evidence is presented which supports and extends the findings of Calabrese (2015), reaffirming the conclusion that the Genetics Panel should be evaluated for scientific misconduct for deliberate misrepresentation of the research record in order to enhance an ideological agenda. This critique documents numerous factual errors along with extensive and deliberate filtering of information in the Beyea letter (Beyea 2016) that leads to consistently incorrect conclusions and an invalid general perspective."

The Cuttler perspective offers an independent appraisal of the Beyea scholarship and finds it "leads to consistently incorrect conclusions and an invalid general

perspective." That is, the Beyea (2016) letter to the editor is not a reliable document, reflecting poor scholarship, bias, and a capacity not to be able to recognize the profound limitations documented in his letter. This is now a well-worn path that Beyea has carved out for himself. These same types of professional failings are glaringly and repeatedly seen in the present Beyea paper and highlighted in this rebuttal.

The primary source approach of Calabrese versus the secondary source approach of Beyea

The exclusive use of secondary sources can be problematic in many ways, especially as the historical foundations of cancer risk assessment extend over a century with about five generations of scientists passing on their modified and imperfect understandings of past findings and how to interpret and apply such information. This process may be affected by cultural changes, language, slight changes in word meanings, education, political perspectives, professional rivalries, funding strategies, and other factors that can and often do change over time. Beyea only taps into the system sometime after original understandings have been translated into secondary sources, not having any understanding of what primary source material existed, what was selected and why, and what was ignored/not selected. Beyea relies upon the authority of other secondary source writers he tends to agree with, rather than checking original sources and determining for himself. For example, the entire BEAR I Genetics Panel and genetics field, in general, accepted Muller's interpretation of the Ray-Chaudhuri dissertation data (Ray-Chaudhuri 1939) that were used to support LNT as published in a non-peer reviewed Conference Proceedings (Ray-Chaudhuri 1944). However, no one learned that Ray-Chaudhuri failed to identify in his dissertation where the locations of the control and treatment incubators were until I reported it (Calabrese 2022a, 2022b). Thus, it is not known what the possible "background" gamma exposures to the control group may have been as noted in my earlier papers (Calabrese 2022a, 2022b). It could have been as high as 24 R. It was necessary to get a copy of the unpublished dissertation and assess the details of the research.

Furthermore, I obtained letters between Muller and Ray-Chaudhuri which demonstrated that Ray-Chaudhuri failed to report key threshold supporting experimental data in the dissertation (Calabrese 2022a, 2022b). The data were provided in correspondence to Muller during the time of his dissertation research. Thus, obtaining primary source material is critical in the search for scientific truth and relates to what is included, what is left out, and why. My efforts have consistently attempted to do this. Beyea has never done this with any of his papers, including the 2024 *HPJ* forum article. When trying to compare

*Exclusive use of secondary sources? Here we have another false premise about my sources. I focus on the **primary** documents that Calabrese tries to discredit, as well as **primary** documents that he does not take into account. Dr. Calabrese has an incorrect view of these concepts. He should have consulted with a historian.*

This is nonsense. Calabrese makes up his own definition of primary sources, which excludes the full scientific record, especially the full chronology.

Views about Ray-Chaudhuri are irrelevant, a distraction from my charges. There is no claim of scientific misconduct around this issue. Calabrese can criticize the science as much as he wants.

Distraction. Not relevant to his charges of scientific misconduct.

*Nonsense. I show that Calabrese overlooks key text in **primary** documents. He ignores my charges.*

Yes, our approaches are different as to professionalism. I find that Calabrese does not operate at a professional level in terms of checking his memory and claims against the primary documents, which leads him to false conclusions as a result of overlooking key text in primary documents, he sets up false premises about historical events. Calabrese should spend more time checking his sources, comparing them to later documents.

Hmm. I have my work reviewed by a historian of science. Calabrese does not. I have been complimented by other historians of science.

Calabrese says it was not worth responding to my second paper, which pointed out many of his failures to carefully read primary documents. For a man who rarely has an unpublished thought and never lets a criticism go by, his claim that it was not worth responding to my 2017 paper suggests he could not provide an adequate rebuttal, just as he cannot in 2026.

Here again Calabrese resorts to falsehood, with his claim that I only rely on secondary sources. The fact that he says that again and again is an indication that he cannot see what I write. It is selective stakeholder perception at the extreme.

Readers should know that I have rebutted these historical claims by Calabrese. He leaves out other correspondence as discussed in Introduction.

More distraction as Calabrese praises his use of a selective set of primary documents. In fact, Calabrese relies on other types of primary documents, just as I do. He will not respond to my examples of his misreading or missing material in the primary documents. Most of his claims about Muller I have debunked , with no response from him. He simply repeats what I have criticized, as if there had been no criticism.

my research approach with that of Beyea on the topic of the historical foundations of cancer risk assessment, it is like comparing apples with oranges. In general, they are not on the same professional level. This comparison is played out in dozens of specific aspects of my research on the historical foundations of cancer risk assessment. The failure of Beyea to research at the most fundamental level will generally lead to an inadequate and uncertain outcome and one that can more readily lead to confirmation bias. It is also frustrating for the reader because Beyea and I are not searching for truth at the same level. I saw this firsthand when Beyea and I had our first professional exchange about 8-9 y ago. I showed how inadequate and often incorrect his analyses and interpretations were. The Beyea approach was inadequate from the perspective of an historian of science and the proper pursuit of truth, and it was not worth responding to his second paper. Now in his 2024 *HPJ* forum article, he continues the same modes of operation, and this has satisfied the inadequate standards of the editorial staff of the *HPJ*.

Primary references are particularly valuable since they can serve to test, challenge, or validate interpretations used in articles that rely entirely on secondary sources. Primary sources can provide information that would not normally be readily available as well as offer critical specific confirming or discrediting insights. As noted above, knowledge of the primary source allows the reader to learn what was known, accepted, left out---and possibly why. This is lost when only secondary sources are used, as is the case with Beyea's approach. Primary sources also provide unique contributions on their own, as my research on Muller and the history of cancer risk has shown. From the first reporting in 2011 on Muller and his Nobel Lecture (Calabrese 2011) to current papers, the extensive use of primary sources has contributed to many of my 150 papers on the topic in unique and substantial ways. For example, the primary literature showed that Muller received and reviewed the Ernst Caspari threshold-supporting manuscript a month before his Nobel Prize Lecture, also praising Caspari as a researcher (Calabrese 2011). A letter 1 mo after the Nobel Prize lecture, Muller confirmed his support for the Caspari findings but stated that it needed more confirmatory research (Calabrese 2011). Other letters showed Milisav Demerec, Director of Genetics at the Carnegie Institution, asking Caspari what can be done to save the "hit" model after seeing the Caspari threshold supportive findings (Calabrese 2015a, 2015b). Other letters have Muller indicating that the Caspari control group mutation frequency closely matched that in his (Muller's) own lab but that the data of Uphoff failed to do so. Based on this, Stern wrote to Caspari indicating that the Uphoff data were no longer considered reliable but that his (i.e., the Caspari data) were now accepted as valid due to the Muller letters (Calabrese 2015a, 2015b).

This is simply revealing a massive string of examples in which my use of primary sources added considerable insight and significance to the Muller and LNT story. The primary sources therefore led me to show Muller's subsequent dishonesty in the early 1950s when he later claimed just the opposite of what he earlier shared in multiple letters with Stern. Muller now conveyed the message that Uphoff's control data, which supported an LNT conclusion, could be trusted but Caspari's control data could not because he characterized it as being aberrantly high. The primary sources therefore caught Muller in a profound deception, but Stern and Caspari were afraid to speak up, to be whistleblowers (Calabrese 2023a, 2023b). Via letters, it was also learned that many radiation geneticists, including James Neel and Don Charles of the Manhattan Project, strongly supported the threshold/Caspari supporting paper of Robley Evans in 1949 (Evans 1949), but they were afraid to challenge Muller publicly (Calabrese & Selby 2023a). The letters also revealed that Muller attacked the threshold supportive studies on mutation of James Neel on the children of survivors of the atomic blasts not only because Muller disagreed with the threshold concept but also, in part, with regard to a battle over potential funding in order to prevent human population studies research from diverting his laboratory grant monies, assuming a zero-sum funding game (Calabrese 2020). This shows that Beyea not only fails to appreciate these and many other previously hidden historical findings but also highlights the fact that Beyea's research never enters the world of primary source research. The contrast between what my work has found and what his few publications offer is striking. There is always a story behind the story that can transform what we believe to be the truth, but Beyea never saw it. The record shows that without such discoveries and revelations provided by the primary source material, the story of Muller's life and the history of cancer risk assessment would be far less insightful, still full of deceptions that the scientific community would think are truths and would still be teaching their students, thereby passing on falsehoods and lies as truths, as has long been the case. The failure of Beyea to use the most reliable historical research methods and documents makes his findings unreliable.

Calabrese selectively used correspondence. Relating to this section and the one above, he left out the correspondence from Caspari after getting a draft of the Uphoff and Stern paper, as I showed in the introduction. He does opposition research as part of a regulatory political campaign to unseat no threshold linearity. One-sided and unbalanced.

This is nonsense, once again. It is classic Calabrese; he claims to be able to get into people's minds. In 2017, I dealt with the larger variance in control data discovered later. Chronology matters.

Evans did not say anything about threshold in the 1949 paper as I point out in Part 2. Calabrese got his premise wrong again. Evans argued that if only a small fraction of the population got exposed, there would be little genetic problem for the population, which would be agreed to by geneticists. As for Caspari, he suggested there might be what we call today a dose rate effectiveness factor. And Evans wrote before the Uphoff replication of the Caspari work, which everyone but Calabrese, including Caspari, accepted as indicating no threshold.

More nonsense about my research. Calabrese's definition of primary documents fails to match that of historians. Calabrese's supposed "hidden historical findings" are usually false findings as I have shown.

Calabrese's arrogance knows no bounds.

Beyea's big mistake: Who do you trust? The historian or the scientist?

In 2017, Beyea noted that he relied upon the "detailed" thesis of Christopher Jolly from Oregon State University (OSU) to guide him on how the BEAR I Genetics Panel assessed and estimated radiation-induced genetic risks. In essence, Beyea decided to place his reputation as a scientist on the experience, technical expertise, knowledge, and judgment of the history of science dissertation by Jolly

Who to trust? That is an easy question to answer. I trust the historians. I show in the introduction and in my annotations that Calabrese missed the evidence that contradicted him about historian Beatty. And his complaints about historian Jolly mirror his error about the nine geneticists or his claim that Jolly missed something in the 1990s, which is irrelevant as well as a false criticism (see Part 2)..

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(2003) rather than digging out the details and evaluating all the potential complexities himself. This was quite a decision by Beyea, with very unfavorable consequences, as will be soon revealed. Jolly obtained the technical reports of the nine BEAR I Genetics Panel members who provided estimates of mutation risks and their methodologies. However, the dissertation offered no technical analyses of how each geneticist derived their estimates, including no critiques or assessments of possible errors, refinements, or limitations of their respective approaches. His analysis was highly descriptive, as one might assume such an historically oriented dissertation would be. On page 319 of his dissertation, Jolly noted that five of the geneticists. (i.e., Beadle, Glass, Kauffman, Crow, and Muller) used Russell's ongoing mouse experiment mutation rate in their estimates. Russell's estimate was also dependent upon it, although he used a different methodological approach.

Jolly's dissertation has a profound omission, which amounted to a major error that his dissertation committee failed to detect and which Jolly, therefore, failed to address. Jolly failed to report that the Russell (Russell and Russell, 1997) (Russell and Russell, 1996) massive mouse radiation mutation study significantly overestimated mutation risks since the control group had a very low mutation rate due to underreporting large numbers of cluster mutations. This omission had been hidden by the Russells for four decades until it was inadvertently discovered and reported to the US Department of Energy. A correction of the Russell findings of 120% (2.2-fold) was imposed by the US Department of Energy (DOE) based on the recommendations of an international expert panel that was convened in 1995 to assess possible Russell research misconduct. The Russells (1996, 1997) corrected this error via publishing the revised/corrected results in the Proceedings of the National Academy of Sciences (PNAS). The whistleblower geneticist, Paul Selby (1998a, 1998b), testified during the Russell proceedings and offered his own technical correction in two follow-up publications, claiming that the Russell errors were even greater than they admitted to, being in range of 5- to 7-fold. The Panel also investigated the Russells for possible scientific misconduct based on arguments that the actions of the Russells were deliberately hidden for decades from Oak Ridge National Laboratory (ORNL) scientists and staff and the broader scientific community since it would have undermined the value of their mouse model, pro- foundly affecting the significance of their research, their very prominent roles in the field, long-term funding of this research program, and the estimation of radiation risks at low doses, which had taken a dominant role. In fact, William L Russell was central to yet another major cover-up, this time with the prominent pathologist and

*Calabrese here returns to my 2017 paper. He again refuses to address my charge that he set up a false premise about the challenge that six geneticists undertook, namely, to calculate **total** mutations after irradiation **and give error bounds**. He mistakenly wants to add different types of estimates with lesser accumulations of mutations into the challenge. He goes on and on, distracting attention from his overlooking the distinction between the types of damage estimates.*

As I discuss in my Part 2, I contest Calabrese (and Selby) as to the implication of background cluster mosaic mutations. Moreover, Jolly's dissertation was about the fallout controversy. The 1956 NAS genetics committee was a part of the larger story, taking up about 1/3rd of the dissertation. Calabrese I found has cited Jolly in some eight of his papers. A connection between Selby's obscure papers written in the late 1990s to the 1956 committee was not made until Calabrese declared such a connection in the 2020s, long after Jolly's dissertation. Calabrese has once again unfairly denigrated someone with whom he disagrees.

I have read the DOE report, which I found did not support Calabrese's interpretation. I also challenge his science, finding it to produce additional false premises. Furthermore, NAS stopped using mouse background values in 2006, finding them to be inappropriate for estimates of radiation-induced mutations (NAS 2006) at pp. 98-101.

Please wait for Part 2, where I contest major parts of this in the Supplement.

future Director of the US National Cancer Institute (NCI), Arthur Upton. In this case, a lifetime mouse study of Russell and Upton showed no radiation treatment effect on longevity and cancer incidence, a striking finding that could have impacted the radiation risk assessment debate in the early 1960s and later years. Russell and Upton ensured that it would not; they suppressed the findings (that is, keeping the data hidden) by never publishing the findings or making presentations at conferences or to appropriate governmental agencies despite the major significance of the findings. The discovery of this story occurred because 33 y after the study had been completed and hidden, Upton was hired as an expert witness for the defense in a major litigation in the UK on radiation health effects. Upton contacted Russell, convincing him to publish the data in a prominent journal since it would be useful in the court case for the defendants. This story became of interest as it emerged in conversations between Selby and me. The details of this story are documented in considerable detail in Calabrese & Selby (2022).

This does not address my charges of overlooking key historical text.

The DOE did not dispute the facts presented at the Hearings concerning the possible scientific misconduct of the Russells, but it did not lead to a scientific misconduct charge, possibly because William Russell was over 85 y old, retired, and with a distinguished career. Nonetheless, the important point was that the control mouse mutation data of the Russells was incorrect to a biologically meaningful degree. It was the incorrect data of the Russells that six BEAR I geneticists used in their calculations that Jolly highlighted and depended upon. If the Panelists in 1956 had used the now-Russell-corrected data, the Panelist risk assessments would have been significantly affected since the corrected findings showed no statistically significant effect, thereby greatly diminishing support for the LNT model.

A distraction. Moreover, the eventual publication of this small sample size study on life shortening did not have any demonstrable effect on cancer regulation.

Besides being another distraction, this statement is false. It contradicts the statement of the investigating committee, which denied any scientific misconduct on Russell's part.

False premise. As I argue in Part 2, the background clusters do not change the Russell estimate of radiation induced mutations. Calabrese thought otherwise due to a conceptual error and failure to check the statistics. Only estimates using doubling dose would have been affected (modestly compared to error range)

The Selby challenge of the Russells therefore proved to be a seminal event in the history of risk assessment. It demonstrated that the dose response of the several-decades-long Russell study, involving up to about five million mice, was no longer supporting a linear dose response model but a threshold and possibly hormetic dose response model. However, this debate was highly technical and handled quietly in high level specialized scientific publications without media publicity, although it certainly deserved it.

Wait for Part 2. Incorporation of mosaic clusters do not change the type of dose response, although they shift it upwards, affecting relative risks only, not absolute risks, if it is decided to include mosaics in the analysis, which is not always done. No threshold. No hormesis. Bad science on Calabrese's part, I argue.

This very notable change in radiation mutation risk assessment was not cited in the Jolly dissertation. Jolly never listed the relevant Russell or Selby publications nor the Department of Energy hearings and its official transcripts, all which occurred at least 5 y before the Jolly dissertation. Thus, the relevant information was in scientific literature and in prominent journals, and Jolly failed to acknowledge and address it.

Selby did not claim threshold or hormesis until the 2020s. The Department of Energy investigation was not made public before Jolly's work. No connection made to fallout or the 1956 committee. Importantly, Selby's work was deemed irrelevant to humans (Sankaranarayanan and Chakraborty 2000) at 115.

Jolly could not see the future.

The Jolly dissertation (Jolly 2003) had a major focus on the BEAR I Genetics Panel and mutation risk estimates and how these actions affected the LNT and radiation risk assessment. Enter Jan Beyea. As noted above, Beyea (2017) proclaimed that he professionally relied on the Jolly dissertation. Beyea also never criticized Jolly for missing the Selby-Russell story and its transformative risk assessment implications. In fact, Beyea also failed to cite the Russell and Selby articles and the DOE dispute in his publications on the LNT topic, now stretching over a decade, including the recent paper (Beyea 2024) in the *HPJ*. Beyea built his scientific beliefs on the compromised Jolly dissertation. Calabrese (2017c) was the first to transform the Russell-Selby story and clarify some of its risk assessment implications, especially with respect to the BEIR 1972 report.

This historical perspective clearly shows that the research methodology of Beyea, which depended on the assessment of Jolly, led to a reaffirmation of an incorrect historical narrative, misleading the scientific and regulatory communities. This error of the BEAR I Genetics Panel impacted many aspects of society, including the emission standards for nuclear power plants and the risk assessment practices of the EPA and regulatory agencies worldwide. The paper of Beyea (Beyea 2024), which depended on the flawed analysis of Jolly, continues to promote and propagate significant errors, showing that it can be difficult for science to be self-correcting.

The compromised Jolly dissertation and its adoption by Beyea improperly affected multiple topics that Beyea included in his recent *HPJ* paper (Beyea 2024). These included the Russell dose rate debate, the BEAR I Genetics Panel activities and its risk assessment applications, the reflections of James V. Neel on the Genetics Panel activities, and the recommendations of LNT for risk assessment and their impacts on the EPA. It also impacted a broad spectrum of his unjustified criticisms of the HPS documentary. Each of these areas has its own story and will be discussed later in this paper. For example, even though there are major discussions about errors and manipulation of data by the BEAR I Genetics Panel and mistakes that Beyea made, his reliance on the flawed Jolly dissertation make many of the follow-up disputes moot since they were flawed from the start, using the incorrect data of Russell as the big driver. The efforts of the BEAR I Genetics Panel were flawed from the start, flaws that Calabrese found and documented. However, these were flaws that Beyea simply copied from the Jolly dissertation and passed them on.

Not only did the OSU dissertation committee fail to correct Jolly on this crucial matter but so too it was missed by the reviewers of the *HPJ* who failed to properly evaluate and judge these significant mistakes of Beyea. The foundational discoveries based on the Selby efforts

Calabrese is back to 2017. And Jolly cannot be criticized for not knowing about Calabrese and Selby's paper that was published long after his dissertation was written. Moreover, Jolly was not making alternative histories; that is Calabrese's approach. And Calabrese and Selby got their math wrong, as I argue in Part 2.

I deal in great detail with this topic in the Part 2 supplement. Wait for it before accepting Calabrese's claims, based on his recent work on this subject, which is irrelevant to Jolly in 2003 and is also wrong, as I argue. Also, BEIR 1972 did not use mice background data.

Not true. False premise. I do not depend on Jolly for anything in the 2025 paper. Not needed to show that Calabrese overlooked key text in primary documents and made a statistical error with A-bomb data.

Not true. False premise.

The Jolly dissertation is only compromised in Calabrese's novel interpretation, which ignores chronology. This denigration of Jolly indicates the absence of substantive response to my charge of overlooking key text in the 1956 report and in the 1949 paper of Uphoff and Stern.

No recommendation of LNT. for risk assessment appears in the 1956 report. This is a major false premise on which Calabrese has relied from the beginning. When Calabrese tries to claim that the 1956 committee influenced the FRC, predecessor to EPA, he cuts off the full sentence that would have indicated that the 1956 committee recommendation was about a recommended dose limit to age 30.

Not true. Calabrese messed up the chronology. The final report dealt with all the concerns of committee members and the uncertainties Calabrese says were ignored, as I have regularly pointed out.

were missed by the *HPJ* reviewers and the associate editor, calling into question the competence of the reviewers and the editorial staff and/or the fairness of the review process. In the end what does this mean for the Beyea paper and the *HPJ*? It indicates that the Beyea paper provided unreliable information on a fundamental area and that then contaminated large segments of his analysis. This was a massive and self-propagating error, like a disease spreading throughout the body, resulting in a high degree of unreliability of the information in this paper, satisfying the criteria of COPE for article retraction. In the end, the decision of Beyea to abandon his responsibility for the papers he published and to depend on an independent third party is simply the wrong and lazy path to follow. In this case, this decision led to the blind leading the blind and then getting opinions accepted by the *HPJ.*

Finally, it should be noted that Beyea (2024) cited the Calabrese (2017c) paper in which I applied the Russell mutation rate correction to the BEIR I (1972) Committee assessment. It is important to note that in this case, Beyea failed to explicitly acknowledge the decades-long error of the Russells, that the Russells-based incorrect data was used by six geneticists on the BEAR I Panel, that the estimated 5 million mutations that were predicted would have been reduced to values not distinguishable from controls, and that his major reliable source of guidance (the Jolly dissertation) failed to report this development and its implications. This gives a further insight into the objectivity of Beyea, which is found to be strikingly deficient and the distorted view of the science that he presented to the readership of the *HPJ*. Surely, the editors of the *HPJ* would have desired that Beyea shared the full story.

All based on false premises and his standard distraction technique of attacking the integrity and competence of those who disagree with him. I do not fault reviewers and editors of Calabrese's historical papers, published in non-history journals, for missing all the mistakes I have identified in his work as described in Parts 1 and 2. I just point them out for readers to assess. They confirm the wisdom of the last NAS committee in its avoidance of citing his LNT papers

False premise about my reliance on Jolly, distracting from his overlooking contradictory text in primary documents. I do cite my 2017 paper eight times in Part 1, but primarily about the studies by the Stern group, which Jolly and other historians do not discuss.

This is mathematically false as I argue in Part 2. The change to background rates does not affect the radiation-induced mutation rate. Although Calabrese has a number of strengths, mathematics and mathematical estimates are apparently not part of them.

Failure to cite critics who were Muller's biographers: What is the point here?

Beyea criticizes the HPS documentary and me for not citing criticisms of Carlson and Schwartz who wrote detailed biographies of Muller (Carlson 1981; Schwartz 2008). Both individuals offered sharp criticisms of me in the journal literature. Schwartz's criticism was part of an interview (Crok 2011), while Carlson offered criticism in the form of a publication with the last section devoted to criticism of me (Carlson 2017). The material presented in those criticisms was itself highly inflammatory, being mostly personally directed and not addressing the substance of my discoveries. These efforts, which are now nearly 15 y old, did not identify mistakes in my papers but rather represented different views based upon one's perspective. For example, an evaluation of the Muller books by these two authors indicates that neither identified any of the information that I presented in my more than 150 publications on the historical foundations of LNT and Muller's role in this process. Given this massive number of

Calabrese is hypocritical to complain about people using inflammatory language. That is Calabrese's trademark as I have shown once again with quotations listed in the introduction.

Calabrese's so-called Information about Muller is not accepted by a single historian. Calabrese is a contrarian in history as well as radiation science.

The lack of citation to Calabrese by these authors is another example of how low is the regard for these claims of Calabrese. They are not accepted by a single historian to which Calabrese can point.

And what dirt does Calabrese think he found in the FBI files related to Muller and the LNT? Surely, he is aware of the known bad quality of FBI files, to which everything is dumped to avoid missing stuff.

Now he goes after the Muller family.

The existence of critical works should have been acknowledged for the benefit of the reader of the HPS article and viewer of the video series.

This is more distraction, with Calabrese ignoring my criticism of his logic, acting as if his words were truth, so any criticism must be wrong. No dishonesties by the 1956 NAS genetics committee, I found. Calabrese's claims are based on false premises that don't hold up when compared to what the report actually said.

As for Detlov Bronk, Calabrese is again attempting distraction, but once again gets the facts mixed up. The report written by a science writer was a summary of six reports, one of which was the genetics report. All were, reviewed by the committee chairs.

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publications and diverse range of criticisms that my publications have directed toward Muller and his colleagues and the radiation genetics field and LNT, one would have thought that the Schwartz and Carlson books would have at least contained brief passing reference to some of these questions and limitations. However, their books are devoid of such information so central to Muller's professional life. How is this possible for award-winning biographers? Thus, my research is as much an illustration of the limitations of their biographies as it is an evaluation of Muller, including his strengths, limitations, and significance.

In addition, Beyea neglects to criticize the authors of these Muller biographies. He simply uses their criticism of me, with no consideration of their limitations. Moreover, Carlson admits in his book that he decided not to obtain the FBI files of Muller, which were likely to contain extremely important and novel information. Why? Carlson did not obtain the information because of a request of the Muller family. What kind of biographer would pass up such information on the subject of the biography? Was there a quid pro quo between Carlson and the family? Yet Beyea fails again to dig into the story. Why? Nonetheless, the Schwartz (2008) and Carlson (1981) biographies are valuable additions to the field despite their limitations in capturing the "real" Muller. However, because Schwartz and Carlson are really part of the "establishment," they get its praise and a pass on their limitations and are given the chance to direct their animus toward me, who criticizes Muller, the hero of their books. That Beyea criticizes the HPS team and me for not citing those works represents a lack of perspective and understanding of this context by Beyea. Schwartz and Carlson apparently know little or nothing about what I discovered about Muller, making their contributions about the topics rather marginal and more background-like.

The same criticisms of Beyea could be offered concerning the debate between the NAS President and me. The President of the NAS, in his rather uninformed attack on me, was compelled to defend the indefensible; that is, Muller's actions and the dishonesties of the NAS BEAR I Genetics Panel and his NAS Presidential predecessors (Calabrese 2014). For example, the past NAS president, Detlev Bronk, lied to the general public when he indicated that the BEAR I Genetics Panel authored the major report to the general public on radiation and mutation risks when it was instead written by a paid third party and never shared with the Panel and contained numerous significant errors (Calabrese & Giordano 2022a, 2022b). The NAS or its president never corrected this false information, and none of the BEAR I Genetics Panel members, including Muller, had the courage of their ethical convictions to challenge Bronk and attempt to set the record straight even though they were aware of the errors

and deceptions and discussed these matters in private letters I have cited. This was another major item discovered by me that was not addressed by Beyea, one of dozens of examples. Again, this was discovered by access to primary sources, that is, the correspondence of the BEAR I Genetics Panel. Therefore, the Beyea narrative is unreliable on this topic as well.

Calabrese continually gets misled by the chronology of his selected correspondence, which makes his interpretations out of date. He overlooks what the final report actually states and how Weaver covered everyone's views and the uncertainty.

Beyea lacks understanding of how the HPS documentary originated, the level of funding, the magnitude of the needed volunteer efforts and rigorous demands

Beyea excoriates Dr. Cardarelli and his colleagues (the HPS Executive Director, the Chair of the Public Information Committee, and Past President Barbara Hamrick) for their efforts to educate the membership of the HPS on LNT during his tenure as HPS president (2021-2023). These individuals invested considerable time in scientific literature preparation, finding the right venue for recording the interview, traveling across the country, conducting the interview (including hundreds of hours of subsequent editing of the interview), and purchased high-end computer and software materials to produce the videos. These are some examples of the level of effort that these HPS members provided to produce the documentary, which brought more visits to their website than any other product ever produced by the HPS. These actions need to be recognized and appreciated because they provide important context, but this was ignored by Beyea. Such treatment was both unfair and inaccurate. The overall unpaid volunteer effort required many hundreds of donated hours. They made a choice concerning how to spend their voluntary time and very limited resources. Furthermore, Cardarelli was in a position to make an informed judgment about the cancer risk assessment and LNT issue because he had a long history of addressing this issue within his job at EPA. He also documented these perspectives in a detailed paper on radiation and cancer risk assessment, a paper that the EPA refused to let him publish. When he eventually did publish it, the agency moved to terminate his employment (Cardarelli 2024; Cardarelli & Ulsh 2018). Cardarelli was well aware of the overriding controversies, opposing views, experts in the field, publication records, and how these developments with respect to LNT developed over time and the role of his professional society in those actions. The Beyea paper fails to present a fair representation of their knowledge, commitment, the quality of their work and their attempts to be fair minded.

No, I criticized them for their failure to educate. If the HPS video series was education, as opposed to an anti-LNT performance, dictionary definitions need to be changed. In the Introduction I gave quotations from the video series indicative of the anti-LN focus.

In addition, when considering the tone of his paper, it should have been necessary for Beyea to point out that I did not seek to be interviewed for the documentary by

Cardarelli knew what he was getting interviewing Calabrese. It is an insult to everyone's intelligence to think that his focus was on being fair as opposed to producing an anti-LNT message that matched his previous positions. There is not a single contradictory view presented. Nor a second source check.

the HPS. I was specifically approached by the HPS to be interviewed. I also played by the rules that were established by the HPS. That is, they would not provide me with questions in advance; I was not permitted to answer questions over again to try to get a better answer or to get it right. I had only one chance at expressing myself on a very broad spectrum of questions. In addition, I agreed to work with the HPS team and provide them with all the documentation that they demanded within a reasonable time. I fulfilled that obligation fully. This information needed to be provided in the Beyea manuscript because it speaks to character, the nature of the HPS documentary, and the lack of rigor and fairness of the *HPJ* peer-review activity.

I deal with this in the introduction. Calabrese provided the documentation, with no one allowed to criticize its applicability or representativeness. As the saying goes, with such rules in the legal system, a prosecutor can get a jury to indict a ham sandwich.

The documentary was a real-time interview without papers in front of me for reference. It was a documentary interview that was based upon my capacity to think in real time and respond to questions that were asked. This is not an easy task for any individual to pull out many thousands of facts from papers that are many years old and some stretching back longer than a century in the historical record. However, Beyea was highly critical rather than again being fair and understanding that this process was extremely challenging mentally and physically and yet I agreed to do this, realizing that 14 h of interviews in such a fashion would have tested anyone's abilities to an extreme degree. Further, I requested no compensation for this major effort nor was any offered. Beyea offers no acknowledgment of this situation. He also fails to appreciate the intense demands that the HPS placed upon me, the person that they chose to interview. While I greatly appreciated their interest in this issue, they were not easy task masters, just the opposite. In fact, it is uncertain how many others would have accepted the terms and conditions of the HPS as I did. Beyea is so focused on trying to discredit me for challenging the status quo that he fails to be fair in his evaluation of me regarding the circumstances and my capacity to respond to the types of rigorous challenges that were imposed on me by the HPS team. Yet, despite all the challenges and demands, the general consensus was extremely favorable and highly regarded, even if information turned the history of the field upside down. The documentary became viewed as a must-see story.

I was aware of the inappropriate burden that Cardarelli put on Calabrese. It is one of the reasons that in my papers, Part 1 and 2, I did not criticize Calabrese's video performance, only his papers. Even with Cardarelli, I only criticized the published paper, not his performance in the video series. Given these rebuttals, however, which claim that I misinterpreted or did not view the series, I have found it necessary to include language from the series that I am sure that even Calabrese would never put in a journal article.

I did not evaluate Calabrese's media performance, only his papers.

Beyea: confirmation bias directs his methods

Beyea makes no effort to address the issue of what his criteria for evaluation were. He made no effort to show the positive and novel findings of my research and its contribution to the field, again showing extreme bias. With respect to the first "Question," he challenges the health physics paper by Cardarelli and colleagues (Cardarelli et al. 2023) because he claims that it is not a comprehensive

What? I said he overlooked key historical text. That was my criteria. Here, Calabrese, who makes rare effort in his historical papers to be balanced, criticizes me for lack of balance. I provided a defense only to provide overall balance.

review of the area of cancer risk assessment and LNT, including more recent findings. He missed the principal point that their paper was a summary of the general perspectives and conclusions offered in the HPS documentary, which was an historical evaluation. The material in this section by Beyea is therefore lacking in relevance and in fairness to the HPS team effort. Beyea then offers a variety of criticisms of my work. However, these are mostly directed toward rehashing criticisms that he provided nearly a decade ago (Beyea 2016, 2017). This is seen in his discussions related to the Manhattan Project experiment with Curt Stern and Delta Uphoff. What is critically missing in his rehash of the earlier papers is the updated findings by me that established that the Uphoff experimentation included two simultaneous variables that made any findings from her experimental work impossible to evaluate from a causality perspective, making this rehashed debate of little meaning. In addition, the data from two of her three experiments remain missing after nearly 80 y (Calabrese et al. 2023a, 2023b). Likewise, he notes the Ray-Choudhuri study that was used to support linearity at low doses, but again he fails to acknowledge recent publications that show serious weaknesses in that research (e.g., the failure to identify the location of incubators for the fruit flies and the inability to quantify potential exposure to the control group which could have been as high as 24 R) (Calabrese 2022a, 2022b). These are examples in which Beyea emphasizes arguments that were nearly a decade old, and he neglects new developments in the field.

False premise, I did not say there should have been a comprehensive review of cancer risk assessment. I said there should have been a statement that the LNT is assessed today with modern data.

A historical evaluation?. The HPS authors' paper was a one-sided historical interpretation that relied on Calabrese's overlooking of contradictory text in primary documents.

Rehashing criticisms? Ones that Calabrese never challenged.

Calabrese does not even today challenge my statement that Caspari's work did not statistically signify a threshold, so that Muller was not wrong to believe no escape from threshold; that Caspari's control group was different than three others and they all were within the variance that they identified and I confirmed in 2017.

Considering Calabrese's claim that Uphoff's experimentation made it impossible to evaluate from a causal perspective: It is silly. Caspari's original study was designed to test dose-rate independence at one dose, 50 R, as can be seen from his paper. The fact that dose rate independence failed with a statistically significant difference, as well as the Caspari results providing only an inconclusive result as to an effect, led to replication. Uphoff's work was a reaction to the Caspari study. I showed in the introduction that Caspari did not doubt her replication results rejecting threshold – results that were published in summary form.

Beyea is wrong and misrepresents the Calabrese evaluation of BEIR I and William Russell

In section 2.4 of his paper, Beyea boldly stated that "Calabrese claims that the national academies committee that authored the 1972 report ignored the dose rate dependency of genetic mouse data." In that same section, Beyea then writes that "Calabrese would have us believe that it was true." Yet, Beyea noted in the previous sentence "how incredible" it was that Calabrese would make such a claim, especially since Russell was a member of that committee. Apparently, Beyea (2024) did not address that Selby & Calabrese (2023) (see page 9, right column) contradicted Beyea's statements by actually quoting from the BEIR I report (1972) (page 65) and discussing the issue that Beyea claims Calabrese says they ignored. The following quote on the topic of dose rate dependency of mouse genetic data---the topic that Beyea claimed that Calabrese said was not addressed. Yet the information now provided shows just the opposite of what Beyea claimed. Not only did I show that the BEIR Committee discussed it, I emphasized it with a long quote, discussed it, and criticized their actions on the matter. The material

The Selby and Calabrese 2023 paper was written after the HPS author's paper and the video series. I deal with it in Part2.

However, I should have stated that it was not dose rate that was claimed ignored, but it was the accounting for female mice that was supposedly ignored, which in fact was handled by the 1972 committee by averaging over the sexes.

Again, I gave the wrong reason for criticism in this case. It was not dose-rate effects that Calabrese wrongly said was ignored, it was the accounting for females, which was handled by the 1972 committee by averaging over the sexes.

Again, I gave the wrong reason for the criticism in this case.

I stated explicitly in my 2025 paper, Part 1, that I was presenting a defense of the early researchers denigrated by Calabrese. I said about the defense: "It is a defense of the integrity of historical figures and committees. I do not expect anyone reading this article to change their ultimate views about the LNT, dose thresholds, hormesis, or supralinearity because of it. My hope is that they will support the addition of errata information and links to critiques on the series website." (Beyea 2025 at p. 507) .

I challenge these ideas in Part 2. Part 1 was already 17 journal pages. No room in it to get into depth about the Russell controversy. In addition, this work of Calabrese's is best reviewed under the statistical error category.

I deal with these papers in Part 2.

from the BEIR 1 (1972) and the discussion is explicitly provided by Selby & Calabrese (2023). Here is the quote used by Selby & Calabrese (2023): "The BEIR I Committee stated: 'The finding of a dose-rate effect for mutation induction in mouse spermatogonia and oocytes raised anew the question of whether there might be a threshold dose or dose rate below which all mutational damage would be repaired. Exploration of a range of dose rates provides no evidence of a threshold dose rate for mutation induction in mouse spermatogonia…Therefore, we shall make the prudent assumptions that there is no threshold dose rate in the male and that the dose response at low dose rates is linear.'"

In fact, that paper placed that quote in a section labeled no less than "William Russell's views on threshold" (page 9, left column). The published statement of Beyea, therefore, is yet another misrepresentation of my research record. Furthermore, Beyea neglected to even cite the Selby & Calabrese (2023) reference that provided the information that discredited his statements. Yet, Beyea is allowed to make repeated incorrect and outrageous claims not only due to a lack of scholarship but also due to the failure of the *HPJ* peer-review process, their lack of knowledge, attention to detail and fact checking, and the lack of oversight at the level of the associate editor and editor-in-chief.

Equally important is that Beyea also missed our complementary discussion of dose rate by the BEIR I (1972) Committee (Selby & Calabrese 2023) that evaluated background radiation dose rate and epidemiologic studies of cancer. This dose rate discussion did not directly involve the Russell research. Thus, Beyea repeatedly missed the key factors leading to his incorrect conclusions.

Making the situation even worse for Beyea, his readers, and the *HPJ* review process and journal system is that Beyea actually cited the paper of Calabrese (2017b) titled "The threshold vs. LNT showdown: Dose rate findings exposed flaws in the LNT model part 2." How a mistake led BEIR I to adopt LNT but then neglected to acknowledge that dose rate was a central component of the paper as may be inferred from the title itself. The paper includes a detailed evaluation of the BEIR I (1972) report, regarding the role of the Russell dose-rate data in the assessment and how the major error of the Russell control group led the BEIR report to profound errors. This paper was the second of a two-part debate over the threshold vs. LNT models for risk assessment and the long-standing debates/disputes between Muller and Russell, culminating in the BEIR I (1972) meeting (Calabrese 2016). Again, Beyea fails to cite the other key papers (Calabrese 2016, 2017c; Selby & Calabrese 2023). In addition, the issues were explicitly discussed in the HPS documentary in episodes 19-20. These episodes not only contradict his statements

but also confirm what is written herein about the Calabrese papers on this topic. These collective failures by Beyea on this topic make his comments about the documentary and my published papers unreliable.

Beyea falsely claims that Calabrese accused BEIR 1 of scientific misconduct

The previous section has demonstrated that Beyea's assertion that "Calabrese claims that the national academies committee that authored the 1972 BEIR report ignored the dose rate dependency of genetic mouse data" was false (Beyea 2024). Further, Beyea ends Section 2.4 (page 10) by stating, "There is no basis for inferring scientific misconduct." This statement by Beyea is alleging that I accused the BEIR I Committee (1972) of scientific misconduct. He provides no evidence to support such an allegation. While it is true that I made such an allegation concerning the 1956 BEAR I Genetics Panel, this was never the case for the 1972 BEIR 1 Committee. The allegation of scientific misconduct is a serious matter, and it is also equally serious when one alleges that someone makes such a claim when it did not occur. The allegation of Beyea not only is wrong but it damages my reputation, suggesting that I made such accusations when they are not justified. The seriousness of this error alone is grounds for retraction. One would have expected that this allegation by Beyea would have been noted by at least one of the reviewers and demand that Beyea provide documentation. However, this was not the case. Likewise, this example represents another serious failure of editorial oversight at the level of the associate editor overseeing the peer review and the editor-in-chief.

Correction of another Beyea error on Muller and LNT: When did Muller start supporting LNT?

Beyea writes that, "By 1948, Muller would drop the 'we believe' caveat in a presentation he made trying to convince doctors to reduce high doses in medical practice, but this was 2 y after his Nobel Prize address." In point of fact, the period between his Nobel Prize Lecture and that quotation was about 3.5 mo (April 1, 1947), not Beyea's 2 y. There was a delay of approximately 2 y before his address was published. It was within 4 mo after the Nobel Prize Lecture that Muller gave a lecture to the New York Academy of Medicine during which he affirmed his Nobel Prize lecture message, stating that there was absolutely no threshold dose for mutations and that induced mutational response was proportional to total dose. His presentation, however, was not published in the Academy's journal until 1948. Thus, it is highly misleading for Beyea to claim that Muller only changed his perspective 2 y later. More importantly, Beyea also fails to acknowledge that in 1930 Muller

Why not address the overlooked historical text? And playing Calabrese's game, why was correspondence not cited from Caspari supporting Uphoff's work and why not the correspondence from Wright that I discuss in the introduction confirming historian Beatty's interpretation?. If one is to rely on correspondence, don't pick a selective set.

I provided in the introduction quotations from Calabrese's work where he did accuse the 1972 NAS committee of scientific misconduct. He must have forgotten. He concedes in the video series that his memory is not great sometimes. True, I gave the wrong reason in my 2025 paper for implying that Calabrese claimed scientific misconduct, but my allegation was correct.

Calabrese is accountable for his words, like every writer. I am accountable for giving the wrong reason for his accusation. I don't think giving the wrong reason lets him off the accusation, however.

I acknowledge the difference between presentation of the talk and publication in Part 2. However, we do not know when the final version was written. In any case, the point is that Muller did not lie in his Nobel Prize address. Also, Muller would have had time to assess the statistics of the Caspari experiment in the period before his talk to the physicians, which he complained he did not have time to do prior to his Nobel presentation.

This is not the point. The question is whether Muller was lying when he stated in his Noble presentation that he believed there was no escape from threshold. This is a key claim of Calabrese's, but he does not address the problem with his claims, changing the subject. Of course, Muller never doubted the non-existence of threshold; he was a true believer in no threshold, just as Calabrese is a true believer in hormesis. The fact that there was insufficient data to prove extrapolation to low doses is not an indication that he disbelieved no threshold based on target theory. I do not read Calabrese's quotation here from correspondence as an indication that he believed in threshold. Calabrese gets the Evans article completely wrong about threshold as I have written, so I would have to read the full correspondence to accept Calabrese's interpretation, based on past experience.

Calabrese has provided his views, calling Muller a liar. Calabrese with some exceptions acts like a prosecutor; I provide a defense for those who can no longer speak for themselves.

Distraction. In any case, I do not mention hormesis in this context. Instead, I said in 2025: "Somewhat ironically, Calabrese himself is not immune to such speculation (Rust 2019), given his funding from industry sources. Fortunately for Calabrese, he can respond to charges about character (Shrader-Frechette 2012), unlike the dead scientists whose reputations have borne the brunt of his unfounded criticisms." Calabrese uses this untruth as an excuse for self-promotion."

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Distraction. Calabrese takes liberties here with what I wrote, which has nothing to do with Calabrese's history on hormesis or when he started his entry into hormesis study and advocacy.

created the original linear dose-response model concept, calling it the Proportionality Rule, with his publications in that 1930 era strongly supporting linearity down to a single ionization (Calabrese 2015a, 2015b, 2019a, 2019b). Thus, Muller is nothing if not consistent in this linearity view, again contradicting the statements of Beyea. In addition, Beyea appears to actually believe in the expressions of support for the LNT by Muller. In this regard, it is fascinating that in private communications on 4 February 1949 with Robley Evans, the highly regarded health physics professor at MIT, concerning ionizing radiation and gene mutations, Muller admitted that "Many of the quantities are only roughly known even for Drosophila, and we are admittedly extrapolating too far in applying this to man…." Muller then advocated for a Precautionary Principle due to the lack of knowledge and/or certainty. Muller asked Evans not to share this information with others, a request that Evans honored (Calabrese 2023a, 2023b; Calabrese & Selby 2023a). While Beyea thinks he knows the real Muller, this private communication suggests that he might not. However, the real point here is that once again, Beyea doesn't even give himself or the *HPJ* readership a chance to wrestle with these questions because his methodology is faulty, and the biased information he provides the reader reflects this. Because the statements of Beyea are incomplete and provide an incorrect perspective, they are not reliable.

Other examples of bias: Beyea misrepresents when Calabrese began research on hormesis

Beyea coyly links my professional interest in hormesis in order to attract funding or other resources, being very opportunistic, directing the readership's attention to the highly critical papers of Rust (2019) and Shrader-Frechette (2010) that I commented on/criticized in the Introduction. However, what Beyea failed to do is inform the readership that I had first discovered the occurrence of hormesis as an undergraduate student in the fall of 1966 and did several years of research on this topic, publishing it in a peer reviewed journal as noted earlier (Calabrese & Howe 1976). This story is now well known. He also failed to indicate that I was invited to present a paper on examples of chemical hormesis at the first ever conference on radiation hormesis in August of 1985 with the peer-reviewed proceedings published in the *HPJ* (Calabrese et al. 1987). Beyea therefore unfairly and incorrectly mischaracterized my entry to this field and how I got involved in it in the first place. This shows that not only was Beyea incorrect, but it reflects strongly upon the bias that he took to this question as this information is widely known and publicly available. It also showed his failure to interview the object of his focus (i.e., me), but he chose to derive conclusions based on,

once again, secondary sources or his own biased assumptions. His information and comments on this matter are therefore unreliable.

Failure to properly represent the body of Calabrese's research on the topic

I have approximately 500 papers published on the topic of hormesis and approximately 150 papers published over the last 15 y on LNT and cancer risk assessment. Beyea should have been fair to the *HPJ* audience in his description of me by informing the readers that my hormesis and LNT papers have been published in the peer-reviewed literature and that I was following and adhering to the procedures and expectations of professionals in the field. That is, I subjected my work and myself to the peer review process with my papers being published in dozens of journals. This means that my work was read/overseen by a large number of different editors as well as being peer reviewed by hundreds of different peer reviewers from numerous countries. This means that I adhered to the procedures and expectations of professionals in the field. Yet, Beyea omits this from his description of me. In fact, Beyea spent much time trying to convince others that I duped the scientific community with my hundreds of papers and presentations because I am such a great communicator and a good writer. While I can thank him for his awkwardly offered praise, the truth of the matter is that the research findings rise or fall on their own. His argument appears to praise my talents but indirectly insults thousands of research scientists that support, cite, and extend my work into their own domains.

Beyea gives the impression that he has evaluated my research in a comprehensive manner. However, this is far from the case. Below are listed more than 60 areas of my research on the topic of cancer risk assessment and LNT not addressed by Beyea (2024), with each having impacts on the historical foundations of cancer risk assessment and the LNT. While Beyea (2024) selects the topic areas for his evaluation, he also needs to place his selection within a professional context with respect to the body of my research in this area, something he failed to do. This affects his capacity to generalize conclusions. Based on my evaluation, Beyea addressed an extremely low percentage of the areas that I have published on the topic, making his efforts nonrepresentative of the body of my work. Thus, his capacity to try to generalize his findings is not supported and therefore his major conclusions are unreliable.

Significant findings by Calabrese not addressed by Beyea (2024)

Muller: Early important findings. Beyea failed to report that Muller's concept of evolution was incorrect.

To interview Calabrese would be foolish, even with a court reporter transcribing the interaction. He takes liberties with whatever is said to shape his own narrative and would use any interview to criticize the interviewer. He is accountable for his written words and omissions, as well as what he says in a public forum like the video series.

Distraction, false premise. I am not reviewing Calabrese's body of work. I am criticizing a subset, namely his historical work on early researchers. Why did he not do this for the past researchers he denigrates? He has not followed procedures of historians.

No, I do not believe that Calabrese adheres to the procedures and expectations of professionals in the field of history, so I could not have told readers that.

Distraction. Calabrese overlooked the section in my 2025 paper: "How did past peer reviewers miss the overlooking of historical text that I have identified in my critique? Calabrese does not publish in historical journals nor let himself be reviewed by historians, so he has no basis for saying that he meets professional standards in historical analysis.

I guess Dr. Calabrese does not recognize that science is contested territory and that sometimes peer reviewed papers are challenged as flawed. He should respond to charges of overlooking text leading to false premises. I deal with post-video series papers in Part 2. He can't even get the date of my paper correct. 2025 not 2024.

Further untruths, distractions and self-promotion. From the beginning of my 2025 article I indicate that I am focused on "the scientific misconduct theories of Edward Calabrese," not hormesis, not chemicals.

Despite his self-confidence, Calabrese is not a universal authority on everything and for every field. He does not get to set standards for everyone, especially when he does not meet them.

Calabrese should have waited for Part 2.

Distraction. This has nothing to do with whether or not Calabrese overlooked key historical text leading him to false premises on which he based his claims of scientific misconduct. Nor does it deal with his and the video series failure to state that the modern LNT is based on modern data.

So what does this matter? Also, disputed. Muller was not the first to find proportionality; he just coined the phrase.

So what does this matter? Also, false premise. I did discuss Muller & peer review in my 2025 paper. Calabrese ignores all the other scientists who published in Science*, such as Morgan and Stadler.*

All of this is irrelevant. Anyway, Muller did not admit what Calabrese claims.

Not relevant, but Calabrese is wrong here, as I argue in Part 2

The rest of this is also irrelevant and a distraction. There are an infinite number of subjects that I did not discuss.

This is all incredibly irrelevant. For instance, gene mutations are not the only form of mutations that cause genetic effects or cancer.

This profoundly affected the development of the LNT-single hit model. It resulted in not incorporating a repair component into the model (Calabrese et al. 2022). This perspective dominated the radiation genetics community through the BEAR I Genetics Panel meetings and is fully documented in the BEAR Panel meeting transcripts.

Beyea did not address how Muller created the Proportionality Rule, its many limitations, and how it affected the development of the LNT-single hit model (Calabrese 2015a, 2019a).

Beyea did not assess the historical occurrence of peer review by the genetics research community during the time of the Muller Nobel Prize paper in *Science* (Calabrese 2019a, 2025).

Beyea did not address how Muller got his Nobel Prize winning paper published in *Science* journal without showing data (Calabrese 2018a; Calabrese 2019a).

Beyea failed to address the seriousness of the fact that Muller's Nobel Prize research was not peer-reviewed (Calabrese 2015a, 2018a, 2019a, 2019b).

Beyea fails to discuss that Muller would admit that he did not induce gene mutations, discrediting the LNT-single hit model that has been used by the EPA (Calabrese 2019a, 2024)

Beyea failed to report that Muller did not induce gene mutations but mostly gene deletions (Calabrese 2015a, 2019a, 2024).

Beyea failed to note that Muller's post-doctoral researcher (i.e., George Snell) was unable to replicate Muller's gene mutation "findings" in mice using the same approach and study design. Muller failed to cite the Snell research, possibly because he knew he was being nominated for the Nobel Prize and the Snell research would damage his chances to win the award (Calabrese & Selby 2024a).

Beyea failed to report that the dose-rate that Muller used in his Nobel Prize study was 100 million-fold greater than background and the difficult issues in extrapolation to background from such a massive dose rate (Calabrese 2019b, 2024).

Beyea failed to note that background radiation could only account for 1/1,300 of the so-called gene mutations in the Muller control group. This means that >99.9% were caused by factors other than background radiation (Calabrese 2019b, 2024).

Beyea did not address the criticism of Muller's gene mutation interpretation by Barbara McClintock that was strongly supported by Altenberg (Calabrese 2019a)

Beyea did not capture the role of Stadler in challenging the gene mutation hypothesis of Muller and how that debate ended; it favored Stadler over Muller (Calabrese 2015a, 2019a).

Beyea ignored what happened when Muller and Stadler worked together to test the gene mutation explanation of Muller via the dissertation of Lefevre. This amounted to the final and devastating criticism of the reverse mutation hypothesis of Muller (Calabrese 2019a, 2022a, 2024).

Why didn't Beyea acknowledge that Muller produced mostly gene deletions? Also why didn't he acknowledge that Muller's close colleagues, such as Jim Crow and others, strongly supported the Stadler position and that it altered their radiation risk assessment views? They concluded that Muller and the BEAR I Genetics Panel had greatly overstated health risks (Calabrese 2019a).

What does that have to do with Calabrese charges of misconduct? Distraction.

Beyea failed to address how Muller and other geneticists worked together to defend Stadler during the McCarthy/Rad Scare era (Calabrese 2017a, 2019a).

Of importance is that Beyea did not report that Muller disavowed the research of Hanson and Heys, while Beyea has relied upon these discredited papers (Calabrese & Giordano 2023) without addressing Muller's comments.

Also, irrelevant, but fails to note that Muller was a competitor of Hanson and Heys.

Why didn't Beyea report that Muller acknowledged in letter communication with Robley Evans that the data supporting risks with Drosophila were inadequately based and that human data were far worse? Evans was asked by Muller not to share this information with anyone (Calabrese 2023b).

Not relevant. Furthermore, Calabrese is such an unreliable reporter of what Evans says in his 1949 Science *article, that no one should rely on his interpretation of an interaction between Muller and Evans without seeing all relevant correspondence.*

Muller and ethics. Beyea ignored how/why Muller took credit for Stern's major discovery of chromosome structure in the mid-1920s. How resolution of this issue led to Muller's participation in the Manhattan Project was not addressed by Beyea (Calabrese 2019a).

Why didn't Beyea assess the role of eugenics in affecting Muller receiving the Nobel prize and the selection of the BEAR I Genetics Panel by the Rockefeller Foundation (RF) (Calabrese & Shamoun 2025)? This is especially important since Muller's receiving the Nobel Prize would have a major impact on the acceptance of the LNT.

The rest of the material on this page is more distraction.

Muller-Ray-Chaudhuri research and LNT. Beyea failed to report that the design of the failed Ray-Chaudhari study was the foundation of the Manhattan Project genetics study (Calabrese 2022b).

Beyea did not report on the letters between Muller and Ray-Chaudhuri showing that the dissertation removed data that supported a threshold (Calabrese 2022b, 2024).

Irrelevant, but Calabrese as I argue in Part 2 has a dismal track record in identifying thresholds.

Beyea failed to report that the Ray-Chaudhuri dissertation did not include the location of the incubators used in his studies (Calabrese 2022b, 2024).

Beyea did not assess the possible dose of radiation in the Ray-Chaudhuri study to the control group and that it could be as high as 24 R (0.24 Gy) (Calabrese 2022b, 2024).

This is all irrelevant to my criticisms. I have never discussed Ray-Chaudhuri; Calabrese does not accuse him of misconduct.

*Distraction, but I dealt with this in detail in 2017, contradicting Calabrese, agreeing with Uphoff and Stern that the **variance** in rates was high based on later data. Calabrese refuses to face the control data values accumulated by 1949, which did not match Muller's early one. There is drift and fluctuation.*

Distraction, but 3 out of 4 measurements by the Stern group disagreed with Caspari's control results. There was nothing wrong with any of them, just variance above sample size variance. They were working at limitations of the methodology.

I have to say that besides being a distraction, this is absolute scientific nonsense. Uphoff replicated Caspari's study and got a different result, one that was statistically significant. This was the importance of her work. Even Caspari agreed this settled the question of there being mutations at 2.5 R per day, i.e., no threshold, as he stated in correspondence.

Besides being a distraction, I can't help suggesting that Calabrese is being hypocritical here. In 2017, after my paper to which he never responded, he published a restating of his views in the journal he edited, with a 4-day time between submission and acceptance. What does this timing suggest?

Perhaps, Stern's communication with Muller, about which there is only hearsay about one side of a phone conversation and only reported by Calabrese, is the reason Muller inserted the words, "we believe" in the sentence in his Nobel Prize talk that Calabrese misreads. Moreover, Caspari's findings were not supportive of threshold; they were inconclusive. Caspari agreed that Uphoff's replication results were conclusive about no threshold (at 50 r protracted exposure).

Beyea failed to report that Ray-Chaudhuri published his dissertation research in a non-peer reviewed conference proceeding, just like Muller did nearly 20 y before (Calabrese 2022b, 2024).

Manhattan Project. Beyea failed to assess how Muller's control group's mutation rate compared with the data of Caspari and Uphoff. It matched closely with Caspari, discrediting Uphoff control group (Calabrese 2019a).

Beyea did not report that Muller completely misled the scientific community several years later by stating that the Caspari controls were aberrantly high and the Uphoff were normal, totally contradicting his earlier written communications with Stern. This raises fundamental scientific and ethical questions that have been ignored (Calabrese 2019a, 2024).

Beyea did not report that Stern, Muller, Uphoff, and the entire BEAR 1 Genetics Panel failed to discover that the Uphoff study had two simultaneous variables that invalidated her study that the BEAR Panel and Lewis depended on (Calabrese et al. 2023a)

Why didn't Beyea report that Stern and Muller and the BEAR I Genetics Panel failed to criticize the Spencer study for combining treatment groups with the same total dose but with different dose rates? Worse still, they depended on these findings (Calabrese 2019a, 2024).

Beyea failed to comment on the ethics of Stern, who published both the Spencer and Caspari papers in the journal *Genetics* while he was the editor and avoided peer review based on the timing of submission and acceptance (Calabrese 2019a, 2024).

Beyea did not report that the key chronic study experimental data of Uphoff have been missing for 80 y (Calabrese 2015, 2019a, 2022a, 2024).

Beyea failed to report that Stern tried to get Muller not to promote the research of Ray Chaudhuri at the Nobel Prize speech but to cite the threshold supportive findings of Caspari (Calabrese 2015a, 2019a, 2024).

BEAR Genetics Panel. Beyea failed to report that at the time of the BEAR Genetics Panel creation, Detlev Bronk was President of the US NAS, President of the Rockefeller Institute for Medical Science (later the Rockefeller University), on the Board of Trustees of the Rockefeller Foundation, and directed the US government's assessment of genetic and cancer risk assessment. Bronk was in a very conflicted position (Calabrese 2014, 2015a, 2019a).

Beyea failed to recognize the importance of the Russell control group error and correct/adjust the BEIR 1 and BEAR 1 mutation risk estimates and their public health and risk assessment implications (Calabrese 2024).

Beyea neglected to report that the BEAR I Genetics Panel knew of significant errors in their report to the public and never offered to correct them. They never publicly corrected that they did not write the report attributed to them. They never challenged the false information by the President of the NAS (Calabrese 2024).

Beyea failed to note that Muller tried to prevent Neel from reporting his findings showing negative results on the induction of birth defects in the 75,000 children he had studied over 10 y after the atomic bomb explosions in Japan and publishing them in a WHO Committee report in the fall of 1956 (Calabrese 2020).

Beyea reported that the BEAR I Genetics Panel reviewed and evaluated the Neel atomic bomb findings when it didn't. Beyea claimed to have access to the BEAR I Panel transcripts when the issue was discussed. This suggests that Beyea did not read these transcripts or ignored the findings (Calabrese 2020).

Beyea did not report that the BEAR 1 Genetics Panel provided the foundations for radiation emission standards for nuclear power plants based on the incorrect findings of Russell (Calabrese 2023a).

Beyea neglected to report that Muller had his close colleague James Crow appointed to the Panel after the Panel had been created to help him in anticipated debates with Dobzhansky concerning his genetic load premise (Crow 1995).

Beyea did not report that the research of Dobzhansky and Wallace challenged the genetic load hypothesis of Muller while still supporting an LNT model (Wallace 1991).

Beyea failed to report that the three youngest geneticists (Crow, Neel, Russell) of the BEAR I Genetics Panel years later disavowed their conclusions and greatly reduced their risk estimates (Calabrese 2024).

Lewis: Major radiation cancer risk paper in Science 1957. Beyea failed to report that Edward B. Lewis did not report the J-shaped dose response for leukemia in atomic bomb exposed populations (Calabrese 2021a, 2021b)

Beyea failed to report that Lewis did not report that radiation induced leukemia in other studied populations was not supported at lower doses (Calabrese 2021a, 2021b).

Beyea neglected to report that Lewis presented key factually inaccurate findings during Congressional Hearings on risks from radiation in 1957. These Hearings greatly affected US policies (Calabrese 2021a, 2021b).

Why didn't Beyea report on the impact of the Lewis paper on the NCRP and their recommendation of an LNT (Calabrese 2021a, 2021b)?

Highly misleading. He has the timing wrong here. This one omission of some words in eight repetitions of the phrase was discovered years after the 1956 report to the public was sent out. And that report to the public was a synopsis of six committee reports.

Distraction. Neel did not have negative results; his results were inconclusive. Muller and Neel disagreed on which data, if any, was appropriate to use for risk assessment.

Once again, Calabrese refuses to face up to the use of the Neel data on A-bomb genetic effects in the actual text of the report. Now, his overlooking of text is willful. See Introduction for more details. Calabrese's findings are wrong.

Distraction. This is Calabrese's story, not Kathren's view. Exposure standards were set 2 years earlier. The Russell findings for induced genetic effects were fine, as I argue in Part 2. Elsewhere in this rebuttal, Calabrese says the committee used the fruit fly, not mouse data.

Distraction plus nonsense.

Total distraction.

Not relevant and misleading. All of these researchers always supported LNT at low doses. Change in risk estimates is normal in science.

No such true response. I challenge this Calabrese claim in Part 2 based on the statistics. Sample sizes too small. Beyond that, Calabrese deleted data points without telling the reader, apparently assuming Lewis would have done the same.

I deal with this in great detail in my Part 2 supplement, arguing that Calabrese is wrong on all counts with Lewis.

Lewis's findings were correct, as I argue in Part 2.

Not relevant to Calabrese overlooking key text and setting up false premises.

Gofman and Tamplin are a distraction.

Distraction. I have not reviewed this paper.

Distraction, if true

I deal with the Selby-Russell debate in great detail in Part 2, arguing that Calabrese makes mathematical and conceptual errors, as well as misrepresenting the DOE report's view of Russell.

This was a small mouse lifespan study, a type that was difficult to do. Calabrese is accuser, not judge.

Distraction. But yes, researchers do change values over time. Moreover, the 1956 Committee gave a range of estimates for doubling dose.

Distraction. Neel's findings as he said were inconclusive, not negative, not an indication of no effects. He looked at only a tiny fraction of genetic effects, limiting the ability to get a statistically significant result.

Distraction.

Distraction.

Nonsense. Calabrese and Selby have their personal interpretation of the scientific record.

Distraction.

Distraction.

Not true. And double take. Calabrese is criticizing someone for a smear campaign? That has been his methodology with early researchers, especially Muller and Stern.

Post BEAR 1 activities

Gofman and Tamplin. Beyea did not report how the Gofman-Tamplin debate led to the creation of the BEIR 1 Genetics Committee in 1972 (Calabrese 2023a)

Beyea neglected to show how Gofman-Tamplin was discovered to be dishonest in public testimonies and its significance (Calabrese 2023a).

Russell. Beyea failed to report that Russell discredited the LNT model based on his presentation in 1970 at a major conference in France (Calabrese 2015, 2019a).

Beyea did not report on the Selby-Russell controversy and the DOE Hearings on the matter and its potential significance for risk assessment (Selby & Calabrese 2023).

Beyea neglected to report that Russell and Arthur Upton covered up a major radiation cancer study and its potential impact on government regulations in 1959/1960 (Calabrese & Selby 2022, 2023c).

Neel. Beyea did not report that both Russell and Neel would later change their radiation doubling dose values to exceed 100 R (1 Gy), directly challenging the BEAR I Genetics Panel estimates. (Calabrese 2020)

Beyea failed to report that the Neel negative genetic damage findings of the Atomic Bombs studies have been sustained for over 70 y (Calabrese 2019a, 2020, 2022a, 2024).

Other. Beyea didn't report that the so-called Gold Standard of the NAS/RERF cancer studies failed to collect smoking data for males for 20 y and for females for 24 y and that the collected data did not appropriately match control group data (Prentice et al. 1983).

Beyea failed to report that background metabolism produces about 200 billion times more oxyradicals than background radiation (Calabrese & Selby 2023a).

Beyea neglected to report that Muller misrepresented the scientific record at the first Pugwash meeting in 1957 (Calabrese & Selby 2024c).

Beyea did not report that multiple studies tested the genetic load hypothesis of Muller, such as the 82 generation study of Spalling, showing no significant effects (Calabrese & Selby 2024b).

Beyea failed to report that the additive to background LNT model assumption was not supported in a large study by Calabrese (2018c).

The Beyea smear campaign method

Beyea attempts to paint me as an industry focused, money-hungry academic. He cites a newspaper article in the *LA Times* (Rust 2019) that led to subsequent publications in *Mother Earth* and *The Huffington Post* that

were highly critical of me. Beyea uses a quote from the *LA Times* that links me to obtaining funding from the tobacco industry. Beyea then tries to be coy in his assessment of this, leaving the question up in the air---but the point is that Beyea tried to associate me with the tobacco industry after implying that I pursued hormesis for financial reasons. This is a type of professional and personal character smear, and it should not have been in the *HPJ* paper. If Beyea had been interested in being fair, he could have interviewed me to get both sides of a story and the reviewers/editor of the *HPJ* should have required it. In fact, I have testified in only one tobacco case, on the side of the plaintiffs, with the plaintiff winning the smoking lung cancer class action case against the tobacco industry. This information was provided during the *LA Times* interview, but the writer failed to include it. This failure of Beyea to seek out information on both sides of an issue shows again the bias that Beyea took to his efforts and that his perspective is misleading and unreliable.

Within this same context, Beyea cited the work of Shrader-Frechette who started her paper about me by claiming that I was responsible for an industry defendant inning a toxic tort case with the family of a deceased worker who died of brain cancer that was associated with exposure to ethylene oxide, a multiple organ carcinogen in animal models. Beyea did not report that Shrader-Frechette failed to inform the readership of the court's official reason why the case was won by the defense. The official court document (Allen v. Pennsylvania Engineering 1996) indicated that all of the plaintiff's expert witnesses were not permitted to testify because they were not deemed as being experts. Their credentials were not accepted by the court. No experts, no plaintiff's case - this is the end of the story. I had nothing to do with that decision. Thus, the plaintiff could not have a trial since it no longer had any testifying experts. The case was dismissed with prejudice without a trial. Again, one finds that a false narrative was provided to the readership instead of what really happened. Yet somehow Beyea's paper passes a clearly biased and ideologically based "fake" academic peer review that lacks professionalism.

Shrader-Frechette also claimed that I hid my industrial funding because an "official" CV on a UMass Marine Science website failed to list any industrial funding (actually it did not include any funding). This was a major point in her paper, and it became important in getting my UMass "inquisition" started based on UMass records. The answer should have been easy. My CV is now over 160 pages with over 1,100 publications and about 800 presentations listed (about 150 pages back then). The UMass group in charge of the website condensed the CVs of all participating faculty into a single page for each person following the same format. Thus, they reduced my CV by

Calabrese says I used a quote from the LA Times that linked him to obtaining funding from the tobacco industry. However, that quote did not appear in the published version. It was in my initial submission, but was removed in the revision. How did Calabrese know what was in my original submission? He also cites my paper as published in 2024, whereas the published version is dated 2025. My submission was in 2024. Yet, he claims that he did not see my paper until it actually appeared in the journal. He has some explaining to do.

Calabrese is worried about being smeared for seeking funding from a tobacco company, but not worried about his smears on early researchers that claim they engaged in scientific misconduct to obtain funding. He says, for instance, about 16 minutes into Episode 15, "It's all about money, money in these these these radiation geneticists." My point in the draft was to suggest that Calabrese was being hypocritical, but I took the quote out because it seemed to be too much like the Calabrese approach that I have criticized. I did not want to appear hypocritical myself.

I actually don't fault him for seeking funding from the tobacco industry, because researchers who are going down a disputed road have a difficult time getting their research funded. But he should not be a hypocrite. Moreover, all researchers have to worry about funding. As I have shown, the geneticists on the 1956 NAS committee chose LNT long before Warren Weaver promised to go to bat for Rockefeller funding. Calabrese and Cardarelli were wrong to suggest otherwise.

Calabrese worries about his reputation, but not the scientists who he denigrates. If you can't stand the heat, get out of the kitchen.

Calabrese filed academic charges against Shrader-Frechette, according to one of her papers, which he does not mention. Quite a scene.

Perhaps, fewer publications would have allowed more time for fact checking, producing less errors.

more than 99%, from about 150 pages to one page. The UMass administrators of that program determined what they wanted, not me. It included my educational background/degrees, job history, honors and awards and areas of expertise and the single page was quickly filled. Thus, the statement of Shrader-Frechette that I hid my industrial funding not only lacked credibility, it was a type of character smear.

Yet, the UMass administration (former Vice Chancellor and my former Dean) became influenced by her writing style, aggressive claims, and perhaps because she was a professor of ethics from the esteemed University of Notre Dame and used her unsupported claims to put me through a major formal ethics and research misconduct "trial." How could this have been possible? How could the administration have been so easily duped? In addition, as these UMass administrators should have known, my UMass funding is publicly available, including from when I started in 1976 as listed in the Office of Grants and Contracts website at UMass, as is the case for all UMass faculty, including theirs as well. There is no way to hide anything.

Yet, Shrader-Frechette's publication vehicle, the journal *Synthese*, printed these false assertions, acting upon these false narratives. Thus, Beyea cites a highly biased article that contained false information. I provided documentation during my "trial" that discredited the assertions of Shrader-Frechette, based on letters supplied by the UMass Marine Science group. They verified how CVs were modified/greatly reduced by their staff and made to be uniform across all the faculty and that I had nothing to do with their process. It was so easy to rebut that it should never have been published or used to impact UMass administrators. Much of the remaining information in the Shrader-Frechette paper followed the same biased path, with some other examples even more egregious than the two listed herein. In fact, I prepared a 50-page document to defend myself in the UMass trial to rebut her allegations, many of which were blatantly false. Again, Beyea failed to do a professional job, relying on false and highly biased secondary sources. Once again, Beyea followed a similar pattern and never made an attempt to interview me. By the way, Shrader-Frechette never interviewed me nor corrected the factual record, a similar pattern of Beyea.

Beyea rehashes the past <u>and ignores current discoveries</u>

Beyea spends most of his time rehashing the past issues of concern to him with almost no consideration of new discoveries made by me since the time of the documentary. I have published about 25 new discoveries with respect to LNT and cancer risk assessment from an

Calabrese feels the victim, taking no responsibility for his role in angering people. However, I have written that Calabrese did not begin his character attacks on historical figures until after his character was attacked. I have always wondered if the timing was somehow causal.

Calabrese cannot run from her articles, just as I cannot run from his rebuttals. It is Calabrese who claims false narratives. If he attacks people's character, he cannot escape the charge of being a hypocrite. I did not support any charges against him. I called the charges speculations. I said:

"Somewhat ironically,Calabrese himself is not immune to such speculation (Rust 2019), given his funding from industry sources. Fortunately for Calabrese, he can respond to charges about character (Shrader-Frechette 2012), unlike the dead scientists whose reputations have borne the brunt of his unfounded criticisms".

Calabrese's pain is unjustified.

The reader does not see what I said, so cannot see how Calabrese sets up one of his false premises, namely, that I had accused him of being a money-hungry Academic.

I cover Calabrese's later claims in Part 2. They fall under the category of statistical errors., the subject of Part 2.

historical perspective that occurred after the HPS documentary. A new paper by me places these 25 new discoveries in a broader and more understandable context since the documentary (Calabrese et al. 2024).

Some of these discoveries directly contradict statements made by Beyea. For example, Beyea cites a 1928 *Science* publication by Patterson (Patterson 1928) as supporting LNT. The paper of Calabrese and Selby (2024d) about Patterson shows that Paterson's claim was not justified. Beyea cites a paper by Hanson & Heys (1928) supporting LNT. He, again, failed to cite a paper by Calabrese and Giordano (2023), which noted that Muller himself in a private letter reported that papers published by Hanson and Heys from the late 1920s into the 1930s could not be trusted because of falsification of the research record. Beyea also failed to cite the paper by Calabrese and Selby (2024a) concerning the research of George Snell, future winner of the Nobel Prize for immunogenetics in 1980, whose work in Muller's lab could not verify Muller's gene mutational findings when extended to mice. These several examples show either that Beyea is unaware of new developments in the field or is biased or perhaps a combination of both. Again, this research required the use of primary sources in order to make important new discoveries. In this case, Beyea failed to discuss our published papers as well as failing to obtain any primary source material, making his conclusions unreliable.

I challenge these claims in Part 2.

Muller and Hanson & Heys were competitors, especially about the experimental support for the proportionality rule.

Not true, as I argue in Part 2.

I cover these papers in Part 2, arguing that, again, Calabrese (and when writing with Selby) overlook key text and numbers, as well as make mathematical and statistical mistakes that overturn their charges of misconduct.

Beyea gets the Neel atomic bomb-genetic effects report study wrong

Beyea also focused on research presented by James Neel concerning the effects of the atomic bomb on the offspring of exposed individuals. My research (Calabrese 2020) showed that Neel gave a copy of his 241-page final report to the president of the NAS and offered it for review to the newly created BEAR I Genetics Panel. It was commented on in that fashion at the opening of the BEAR I Genetics Panel (November 1955). Yet, under the leadership of Muller, the BEAR I Genetics Panel never evaluated it because Muller publicly stated that its findings were illusionary (Calabrese 2020). Beyea fails to bring this up in his paper. Due to Beyea's lack of use of primary sources, he also failed to share that Neel would subsequently send the key final chapter of his major report to all the members of the BEAR committee so that they would at least be aware of it. The key information of the Neel study therefore was never discussed by the BEAR 1 Genetics Panel. This led to angry major public disputes between Neel and Muller in the summer of 1956 at a conference in Copenhagen that was written about in the *New York Times* and within a subsequent WHO meeting as discussed below. The details of that key episode are now summarized below.

This distracts from the fact that the 1956 genetics report makes quantitative use of the Neel data, as well as correctly discusses its sparsity.

Muller was not the leader. How can Calabrese expect to get away with such nonsense? Muller made one disparaging comment in a meeting. However, the data were analyzed, e.g., by Sewall Wright, and the results for doubling dose, as good as could be made, appeared in the report. Calabrese cannot face this contradiction to his rhetoric.

The Committee included Neel's results in their report. No stomping of the feet can change that and reverse the, false ink spread by Calabres on this subject.

The Muller Neel dispute was about which data, if any, are best to use. Science is contested territory.

Yes, Calabrese published a study in 2020, but it was wrong about the Neel study and the evaluation by the 1956 genetics committee. This is one of his classic overlooking of key historical text that appears in the 1956 report. Moreover, as I show here, Calabrese overlooked correspondence where Sewall Wright uses Neel's data to quantify a bound on the doubling dose of radiation.

Now, Neel did not think the animal data, especially on fruit flies, was translatable in a quantitative sense to humans, which he debated with Muller, but he did not doubt that radiation produced mutations in humans. Eventually, he extracted a number from the A-bomb data, which is about 4-5 times less than estimated from fruit flies, not a threshold..

Here is a key Calabrese mistake. He will not face up to how the final report presents the Neel data and how Sewall wright did calculations about the doubling dose using Neel's data. The gold standard for the words in the committee's report is the report itself, Calabrese refuses to face up to the two places in the report where this calculation is discussed. I find this to be incredible. He ignored my argument in 2017 and today. Neel signed off on this calculation by signing onto the report, another indication beyond his article writing that his data did not mean threshold. Calabrese sees the data as showing a threshold, as I have discussed in both Parts 1 and 2l, but that is a statistically incorrect inference.

The BEAR committee has no formal recommendation about LNT, only exposure standards. Calabrese never admits this. In general, the transcripts in full do not support Calabrese's interpretations based on his excerpts as I have argued. What the recommendations were based on is a distraction from Neel saying "...the data from human studies playing at that time—properly—a minor role.

I have never been on an NAS committee where members did not disagree on issues, disagreements that had to be resolved one way or another in the drafting process to prevent minority reports, which are feared because they get all the attention, detracting from the much larger number of agreements.

Distraction. Science is contested territory.

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Part 1---The Panel did not evaluate the Neel study data: Here is the story

In 2020, I published a detailed paper that explained why and how it came to be that the major genetic damage study of Neel and Schull, which became ready just at the time of the BEAR I Genetics Panel, was unceremoniously brushed aside (Calabrese 2020). However, Beyea (2024) states on page 5 (left column) that the BEAR 1 Genetics Panel assessed the major study of James Neel and his colleague William Schull on the genetic effects of the atomic bomb blasts on the offspring of exposed Japanese adults. He challenges my conclusion that the Panel chose not to assess Neel's findings due to the powerful influence of Muller. Beyea then states that "the principal sources of data for these recommendations were the mouse experiments of WL Russell, the data from human studies playing at that time---properly---a minor role (see Neel 1994)." Let us examine the facts.

We will find that Beyea got the issue wrong because he failed to properly assess the primary source of information, the BEAR I Genetics Panel transcripts; that is, the Gold Standard and related Panel letter exchanges. The BEAR I Genetics Panel did not assess the major Neel & Schull (1956) study that was handed to the Panel on a silver platter by Neel at the time of the very first meeting, thanks to Muller's influence. Secondly, both Beyea and Neel got it wrong. The BEAR I Genetics Panel recommendations (e.g., LNT) were not based on the Russell mouse data but on the historical reliance on fruit fly data and Muller's major evolutionary biology mistake when he assumed that there was no such thing as genetic damage repair (Calabrese et al. 2022). It is clear that the memory of Neel, now some four decades after the BEAR I Genetics Panel meeting, was faulty as shown by the transcripts. Beyea accepted the memory of the aged Neel rather than carefully checking the transcripts. Beyea made the same mistake of not checking the transcripts with Bentley Glass in his earlier paper as noted above (Beyea 2016). I was able to show definitively that Glass's memory was also faulty, again by using the transcripts of the BEAR I Genetics Panel (Calabrese 2017b).

The Neel story describes not only that incident but also that it led to a major battle between Muller and Neel that erupted 3 mo after publication of the BEAR I Genetics Panel Report in June 1956. The "explosion" occurred at a WHO meeting (First International Congress of Human Genetics) in Copenhagen, during which Muller and Neel got into such a conflict that it led to the *New York Times* writing a story about the battle between the Mullerians and the anti-Mullerians (i.e., the followers of Neel). The fight was over the use of Neel's epidemiological data for human risk assessment. The battle of the two titans of genetics spilled over the next week at a WHO Study

Group meeting in Copenhagen. At that meeting, Muller tried to prevent Neel from speaking and contributing his paper to the Group Proceedings. Neel was saved when the British contingent threatened to boycott the meeting. The hostilities continued in the US as Alexander Hollaender hosted a failed "reconciliation" meeting. The point of this brief summary is to show that all was not well with the BEAR I Genetics Panel as a result of Muller using his power and influence to block the use of the Neel & Schull (1956) study from use by the Panel. Just think how most researchers would react if their 10-y magnum opus on human genetics was blocked by a fruit fly researcher. The reader will see that Beyea's belief that the Panel reviewed the Neel & Schull (1956) report is an illusion. This battle and severe acrimony between Muller and Neel never would have occurred if Muller hadn't yanked the study from the Panel with Neel never anticipating that anyone would have done such a thing.

Here is the summary story [page 3, left column of Calabrese (2020)]: Based on transcripts of BEAR 1 Genetics Panel meeting, in opening comments on November 20, 1955, NAS President Bronk circulated among the Panel members 10 NAS reports concerning radiation, including an earlier version (date not identified) of Neel's ABCC atomic bomb offspring survivor study. After the statements of Bronk, Panel Chairman Warren Weaver inquired concerning the availability of Neel's updated and expanded Atomic Bomb Casualty Commission (ABCC) offspring survivor-genetics study. Shields Warren, who was both a BEAR 1 Genetics Panel member and chair of the ABCC, stated that Neel's major report would be available soon. Later in the meeting, James Crow would state, "We need to know more about man himself, about the effects of radiation." This comment was reinforced by Tracy Sonneborn, who stated, "I agree with Crow that we need intensive efforts to acquire information in regard to man. No amount of extrapolation is as relevant as the direct information on man himself." However, Muller soon put a halt to this discussion, focusing on human data with his statement that "We should be aware of reliance on illusionary conclusions from human data, such as the Hiroshima and Nagasaki data, especially when they seem to be negative." Following that exchange in the panel meeting, the issue of Neel's Hiroshima and Nagasaki study and his apparent "illusionary conclusions" were never discussed. At that key time during the meeting Neel was silent, letting Muller's criticism stand. Later that same day, Neel made a very limited rejoinder, indicating that "the proper study of mankind is man" but with no specific application to the Muller criticism and no follow-up action. The issue therefore of the availability, use, and importance of Neel's Hiroshima and Nagasaki study was never again brought up for discussion. It was also not included in the meeting summary and the Panel's goal statement by Weaver.

Muller did not block the use of the Neel data. The implications of the data for the doubling dose appeared in the 1956 report, which Calabrese never mentions, and Neel's concern that animal data were insufficient to translate to humans is also recognized: "Also they recognize that there is a risk involved in extrapolating from mouse and Drosophila data to the human case." At p. 1163. Calabrese refuses to mention these facts. He has no defense beyond attempts to distract the reader from my charges.

What happened after the report in 1957 is a distraction. Muller and Neel continued their debate. Scientists disagree. NAS studies cover all points of view to avoid minority reports. Neel admitted he was in a minority about there being too much uncertainty to make quantitative estimates, but insisted his view be noted, which it was in the final report

Neel had published preliminary results in 1953 before the 1955 and 1956 meetings. He gave a presentation in 1954, which was described in the first meeting (check which one). His results were similar in 1956, although he made a plea at the end of his 1956 report (Section 15.1) to not make quantitative estimates. His view was included in the report.

These committee members, of course, wanted human data, but it was not yet available. They all knew the limitations of the Neel data but thought animal data was useful, unlike Neel.

How exactly did Muller put a halt to this discussion? Did he threaten them? How did Neel's data about doubling dose appear in the final report? How did Neel's views about the inappropriateness of calculations with animal data appear in the report? Calabrese has no answer.

It was never discussed subsequently in the available transcript, suggesting that no committee member other than Neel disagreed sufficiently to comment. Neel himself said in his autobiography that in this period, the A-bomb data was not sufficient for use in decisions. Sewall Wright discusses the Neel data in subsequent

correspondence with chairman Weaver..

Neel did not think fruit flies or mice were adequate for assessing human risks, a direct difference with Muller. The two of them had much correspondence about this.

But others made sure that the implications of the Neel data for doubling dose was in the final report, which Calabrese cannot face.

Neel's opinion was noted in the report, but he was in a minority as to the usefulness of estimates made with animal data.

Calabrese overlooks here the fact that the LNT dose response was part of the agenda of the first meeting in 1955 and was taken for granted by several speakers in the early discussions of the first meeting. Later in this section he concedes the latter, contradicting his claim here that Sonneborn's summary was significant. Cardarelli uses this to claim that Weaver's offer to go to bat with Rockefeller Foundation brought the Committee to adopt LNT as the science, which fails a chronology check.

It is crucial to emphasize that in November of 1955 (exact date unknown) Neel indicated that he had sent Muller a mimeographed copy of the updated and expanded Neel-Schull (1956) report on the ABCC atomic bomb study. While Neel may have been under the impression that Muller had read the report, Muller (1956) noted in a letter to Neel (24 October 1956) nearly a year later that he had never read this report, being too busy (Calabrese 2020). Muller's dismissive and critical comments about Neel's report during the opening of the BEAR I Genetics Panel session were therefore speculative, premature, and judgmental, as Muller had admittedly never read the report and was not knowledgeable of its contents.

After those disappointing developments at the meeting of the BEAR I Genetics Panel, Neel wrote to Weaver with a copy to Muller on 23 January 1956 (Neel 1956a), stating that, "As you know, for some time now, I have been deeply involved in studies on the potential genetic effects of the atomic bombs in Hiroshima and Nagasaki. Dr. Wiiliam J Schull and I have been working forthe past two years on the report on this experience and expect to get the final manuscript off to the press by April 1, 1956." That last chapter of the Neel report (i.e., Chapter 15) (Neel & Schull 1956) indicated that the state of current knowledge regarding radiation-induced genetic damage of humans and any other animals was so inadequate that speculation concerning the long-range effects of radiation on the genetics of the human population was, according to Neel, "...to say the least, extremelyrisky." Even though Weaver strongly desired specific, quantitative risk assessment guidance, Neel emphasized that the human data from his ABCC research as well as the animal model data did not support this. In contrast, Muller indicated that any estimates that would be provided by the Panel would ignore Neel's study.

The next meeting of the BEAR I Genetics Panel was held on 5-6 February 1956. The most significant development during that two-day meeting was the adoption of a strong consensus that emphasized the belief in the linear no-threshold dose response for radiation-induced mutations based on a presentation by Tracy Sonneborn. At the end of the meeting on 6 February, Weaver challenged the geneticists to estimate at least three transgenerational adverse genetic effects that would occur in the US population, including those starting with the next generation (F1) and then out to 10 generations should the parents of the current generation be exposed to 10 R of ionizing radiation or if the exposure were 10 R for each generation. The imposition of the LNT assumption by Weaver was designed to enhance a convergence/agreement of damage estimates among the panelists, eliminating a major source of potential interindividual variation in predictions. Each geneticist on the Panel was to construct his own independent damage.

estimation for the questions posed by Weaver and then send via US mail a detailed report before the next meeting on 1 March. In fact, three of the geneticists refused to participate since they thought that reliable estimates could not be provided. The science was not yet good enough. One of those refusing to participate was James Neel. He provided his multiple scientific reasons in letters to Weaver, which I have documented (Calabrese 2020).

This proposal of Weaver concerned Neel beyond his refusal to participate, prompting him to write to Weaver on 14 February 1956 (Neel 1956b), in which he stated, "You will recall that I stated at the recent meeting my conviction that our knowledge was far too fragmentary to permit a meaningful quantitative statement of this problem. My reasons for this are spelled out in some detail in Chapter 15 of the monograph which Dr. Schull and I are now preparing on the Japanese study. Although there are dangers in presenting this one chapter apart from this entire monograph, nevertheless I have decided to send a copy of this chapter to every one of the committee as soon as possible, probably within the next two weeks. While I do not expect to make many converts, this will perhaps make the reasons for my stand somewhat clearer." On 21 February 1956, Neel followed through and sent a letter to the BEAR I Genetics Panel, attaching Chapter 15 (Neel 1956c). He stated that, "You will recall that at our recent meeting in Chicago I expressed certain reservations concerning our ability to develop worthwhile predictions concerning the genetic effects of radiation. The paper supplies at least part of the details on which that position is based."

Together the transcripts and summary written by Bentley Glass (Glass, 1956) provide a more substantive reconstruction of the panel meeting on 5-6 February 1956. The Glass write-up matches closely in content and time with the transcripts but not fully. For example, Glass noted that in the meeting, Neel said he would have Chapter 15 of the study mimeographed and sent to all members of the panel. However, according to the transcripts, Neel never made such a statement in the meeting. Neel may have mentioned this to Glass during a break or after the session had ended or not. As noted above, Neel would later write to Weaver on 14 February 1956 (Neel 1956b) about sending Chapter 15 to the entire panel. The 241-page report by Neel & Schull (1956) contained highly detailed research chapters on each end point and contained sections on research design, study methods, results, discussions, and perspectives. No statistically significant findings were reported for any of the end points.

Even though the study of Neel & Schull (1956) was a major development, there is no evidence that the NAS leadership made it available to the panel. In fact, the evidence contradicts this position. Neel only made Chapter 15 available, as it was focused on risk assessment issues,

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Calabrese doesn't get to choose which group of the disagreeing geneticists were right.
And Weaver noted Neel's concern in the final report in a way that allowed Neel to sign on.

Distraction. Other members, animal geneticists, who dominated the committee makeup, believed that their data was relevant to humans and they were willing to try to make estimates, some of different types, while noting the uncertainty and the too many assumptions needed to make. Neel was in a small minority about this issue.

So, Neel circulated the chapter in his report that outlined his concerns about making estimates. So how does Calabrese claim it was not considered by the Committee? There is scientific disagreement here, not scientific misconduct.

A lot of this A-bomb data had been previously published. The new data were more complete, but results were the same. Calabrese has not indicated how the latest report had any new information relevant to the Committee beyond what the Committee's report includes about the human doubling dose limit and the reservation of some geneticists that numerical calculations based on animal data were not appropriate for humans. The 1956 study was incremental, but for the last chapter, which warned against numerical calculations at this early data. It was circulated. He did not expect to make converts.

Distraction. It was only the last chapter that was new. It was a pitch for not making quantitative calculations at this time, which other committee members did not accept, as Neel figured they would not. After all, most of them were animal geneticists who believed in the value of their work.

Calabrese overlooked correspondence to Weaver by Sewall Wright in which he estimates a limit to the doubling dose using Neel's data, as I discuss in the introduction.

Once again, Calabrese is distracting readers by ignoring what the report actually said quantitatively about Neel's data and the caveats about risk calculations by the geneticists that also appear in the report.

Distraction ad nauseum and wrong.

some two months after the Panel started their work, that is, in the second half of February 1956.

Yet, Neel's actions as seen in his 21 February 1956 letter (Neel 1956c) to the Genetics Panel shared only one of the 15 chapters. His memo expressed little, if there was any urgency but only a desire to be better understood. Further, the Panel members had prepared and sent their specific estimates of genetic damage to Crow before or about the time of receiving Chapter 15 from Neel. Although Neel could have used the next meeting to explain the negative findings of the atomic bomb study and his contrary position, this never happened. During the follow-up 1 d meeting on 1 March 1956 in New York City, six geneticists, including Neel, did not attend, thereby precluding discussion of his ABCC study findings and its potential for substantially impacting the Weaver assessment memo. In fact, a review of each of the assessments submitted by the 9 of the 12 geneticists who provided an independent written estimate reveals that none cited the material provided by Neel.

This reconstruction of the BEAR Genetics Panel activities within the Panel meeting activities demonstrates that Beyea is incorrect in his representations and failed to share this information with the readership. Beyea claims that the "...data [of the Neel study] were considered but Calabrese apparently did not find it." The above reconstruction indicates that the Neel report was available but was not shared and considered by the Panel. In fact, Neel was so concerned that he himself, not the chair of the Panel, even personally sent the Panel members Chapter 15 (only) toward the end of February. The evidence indicates that the receipt of the chapter did not affect any of the nine reports that addressed the Weaver assignment given on 6 February 1956. Weaver provided a follow-up letter (8 February 1956) (Weaver 1956a) to the Panel, clarifying the instructions for their risk estimation project. This story was spelled out in detail in the Calabrese (2020) paper, which Beyea cited but failed to reliably report.

Part 2---How LNT was adopted by the BEAR I Genetics Panel

This section shows how confused Beyea's understandings are with respect to how the BEAR I Genetics Panel derived the LNT. In many respects, it is a complex mixture of misunderstandings and mistakes that indicate that Beyea lacks an understanding of what he is alleging. These errors are now presented from two differing perspectives, which I am calling for simplicity's sake Beyea Mistakes #1 and # 2. Beyea has accomplished the seemingly impossible, creating two distinctly different mistakes on the same topic but via differing approaches and assumptions.

Beyea mistake # 1. Beyea makes the conclusionary statement that the LNT-based hereditary risk assessment for ionizing radiation was based largely on the mouse research of William Russell (Russell 1951). He based this conclusion on the nearly 40-y-old memory of James Neel, as given in his autobiography (page 321) (Neel 1994). The problem with this appeal to authority is that Neel's memory had faded and that Beyea had failed to check the transcripts of the BEAR I Genetics Panel meetings, especially on 5 February 1956, the first day of major Panel work after an introductory meeting the past November. In this telling meeting, the transcripts reveal that Tracy Sonneborn wrote a general guiding statement of principles for the Panel to follow. The framework of Sonneborn was based on four principles: i.e., that all doses of ionizing radiation were (1) harmful, (2) irreversible, (3) cumulative, and (4) displayed a linear dose-response relationship. The presentation by Sonneborn at the meeting did not provide reference citations. No member of the Panel challenged these perspectives. In fact, at the first meeting of the Panel back in November, Sturtevant asserted his scientific contempt for the medical profession that still strongly supported an "anachronistic belief " in the threshold dose-response model. Sturtevant stated that he had "no doubt about the correctness of the linear dose response." He also stated that any effort to further document support for it would only be for the propaganda value in order to "educate and convince the non-geneticists." The Panel's single-minded uniformity of belief regarding the nature of the low-dose response was profoundly significant as it tended not only to limit discussion and preclude debate but also to ensure adoption of their preconceived beliefs.

The apparent uniform belief was derived from the proposal of Muller in 1930 for a Proportionality Rule for the dose response for ionizing radiation and gene mutation (Calabrese 2015a, 2019a, 2019b; Calabrese & Selby 2023c; Calabrese et al. 2022). This Proportionality Rule was a precursor term for the LNT-single hit model that contained no genetic repair component. This idea was based directly upon Muller's belief that natural mutations were so infrequent that evolutionary novelty could not occur if nature had evolved the capacity to repair induced genetic damage. This was based on large scale observations of fruit flies in which only about 400 visual mutations were observed from observations of about 25 million flies. Thus, the original thinking behind the linear dose response was derived from Muller and his research with fruit flies. Later studies with fruit flies were used to support this conclusion, including some studies with fruit flies by Hanson & Heys (1928)[2], Oliver (1930), Spencer & Stern (1948), and Uphoff & Stern (1949). None of these

[2]Muller considered the Hansen and Heys research to be fraudulent as discussed and cited in this article (see Calabrese & Giordano 2023).

This is strange, because Calabrese himself has stated that most of the estimates made by the geneticists who accepted the total mutant challenge used the Russell 1951 data. His latest paper with Paul Selby confirms it.

Now Calabrese shifts from the 1951 data used in quantitative estimates to the general idea of linearity and gets the timing wrong.

The LNT assumption appears in the agenda for the first meeting, before Tracy Sonneborn read his draft statement in the second meeting. Sonneborn summarized the consensus. The correctness of linear no threshold was discussed by a number of members early in the first meeting, as Calabrese concedes later, with no dissent. No geneticist on the committee ever expressed doubt about the LNT, certainly before or during the 1956 meeting. Calabrese has never provided any such evidence. There were only differences as to the slope and seriousness of the effect.

So, Calabrese here concedes that there was uniformity of views about no threshold, which means that chairman Weaver did not influence views on LNT.

Distraction about the proportionality rule. In any case, the idea of no threshold did not come from Muller alone. The LNT was expected from the nature of the rays of radiation (quantum hit model) based on the physics of irradiation and its quantum nature (COMPTON and Allison 1922) (Lewis 1926). Muller acknowledges the expectation of no threshold, "if the effect is a direct effect, locally produced by the electron hits..." (Muller 1930) " However, Muller wanted conforming data. Hanson and Heys claimed to have found confirmation first but agreed that Muller coined the term proportionality rule (Hanson 1933).

So, Calabrese is here conceding that the LNT was discussed and accepted by members, and not just by Sturtevant, before the second meeting in 1956.

Yes, the committee accepted LNT before Weaver offered to go to bat with Rockefeller if a case could be made for research.

This was the state of genetics science in 1956. This is more distraction.

Everyone agreed to LNT, but Muller was only one of many players. Hit theory and the experience from looking under the microscope at the behavior of radiation-induced genetic aberrations was influential.

In fact, most of those undertaking the total mutant challenges used Russell data. So what?

The committee members were not trying to prove LNT for genetic effects. They assumed it.

*Major Calabrese flaw: Only six geneticists, not nine, took up the **total** mutant challenge with uncertainty ranges.*

The Committee made no formal recommendation to adopt LNT. This is one of Calabrese's embellishments and his taking liberties with language. It was among the six estimates where the Russell 1951 data was used.. In any case, this is another distraction.

No, I got the date wrong, but I was not substantively wrong. Neel's statement about 1960 also applies to 1956, namely that the data from humans played at that time – properly – a minor role. It demonstrates that the 1956 report handled his data properly.

studies were strong enough to test the LNT assumption in the low-dose zone. This dominant perspective of Muller was still quite apparent at the BEAR I Genetics Panel, as seen once again on 5 February 1956, with Sonneborn who stated that, "Ordinary consideration of life inescapably involves exposure to irradiation and other mutagenic agents, quite apart from the additional exposure due to the atomic age, medical use of irradiation and man-controlled superimposed mutagenic agents." Based on this framework, Sonneborn further stated that, "Inescapable mutation provides an ample means for evolutionary advances for genetic adaption to changing conditions of life. It also involves mainly genetic damage under present conditions. Additional mutations only add further damage without materially increasing the capacity to adapt and evolve. Given inescapable mutations, genetic adaption and evolution depend upon selection, not upon more numerous mutations." These perspectives can be traced based to the LNT single-hit paper of Timofeeff-Ressovsky et al. (1935) that formulated the linear dose response single-hit model. Thus, the historical foundations of LNT were established in the belief system of the radiation genetics community under the leadership of Muller and others for at least two decades prior to the creation of the BEAR I Genetics Panel.

With respect to the mouse research of Russell (1951), it gets introduced into the discussion only after Weaver challenged the geneticists of the Panel to estimate genetic damage from exposure to 10 R from 1 to 10 generations, under the assumption that the LNT model was operating. Nine panel members took up this challenge using the models with which they were most familiar, including bacteria, fruit flies, mice, and humans. Thus, the challenge of Weaver built in the assumption of linearity so that none of the applications used by the Panel members were designed to prove LNT, which was the most fundamental focus of the Panel. They only used it after it was given to them for application. The Russell data had no impact on the adoption of the LNT model recommendation of the BEAR I Genetics Panel. Thus, the conclusion of Beyea is incorrect.

Beyea mistake # 2. There is yet another mistake that Beyea made with his interpretation of the Neel (1994) book concerning the role of mouse data in the 1956 BEAR I Genetics Panel Report. Beyea quotes from the Neel book on page 321: "The principal sources of data for these recommendations were the mouse experiments of WL Russell, the data from the human studies playing at that time ---properly—a minor role." However, a reading of the Neel (1994) page 321 reveals that Beyea got the story wrong, totally wrong. The Neel paragraph begins stating, "Our Committee was reconvened in 1959. There

had been one extremely important genetic development since our original charge." The one major new discovery was that of Russell et al. (Russell et al., 1958), who published a groundbreaking paper on 18 December 1958 in the journal *Science*. It showed for the first time that dose rate rather than total dose predicted the mutational damage in mouse spermatogonia and oocytes. The findings suggested the existence of DNA repair. The findings were so important that Russell was nominated several times for the Nobel Prize. The findings, in fact, refuted two central claims of the 1956 Genetics Panel report that all radiation-induced mutational damage was cumulative and irreversible. Russell contacted Muller several weeks before the *Science* publication out of respect and to give him a heads up on these findings. These findings were commented on in the next BEAR Genetics Panel report in 1960. It summarized in a few sentences the Russell findings and that these findings had been confirmed with Drosophila. This confirmation was done in Muller's laboratory. Despite the striking findings and that they had discredited the cumulative dose and irreversible damage belief, the Panel felt that the quantitative features of these new findings remained to be clarified and no changes in guidance were offered in their 1960 report. The principal point here is that Beyea related these major findings of the mouse data of Russell et al. in 1958 back to being the basis of the recommendations of the 1956 report, which they weren't. In fact, the Russell et al. (1958) findings were discrediting the 1956 BEAR I Genetics Panel findings and their recommendations. Thus, Beyea did not understand and appreciate or perhaps even know of the findings of Russell and Muller, their timing and how they were evaluated by the BEAR Genetics Panel in their 1960 Report. Plus, Beyea incorrectly claimed that these findings were the basis for the 1956 recommendations when they could not have been and, in fact, discredited them. In addition, they did not even affect recommendations in 1960.

The entire story of the 1958 Russell et al. findings, how Muller replicated these findings, and how they impacted the 1960 BEAR report were the main focus of Episode 19 of the HPS Documentary. This suggests further that Beyea never watched Episode 19 where the story was told in great detail, sharing multiple primary documents that verified key aspects of the above summarized story. Beyea's assessment of the role of the Russell mouse data for the BEAR I Genetics Report is shown to be incorrect and thus unreliable. Beyea also failed to understand the paragraph on page 321 of Neel (1994), creating even more errors in his paper, making this section also unreliable.

Beyea continues to misread the Neel autobiography creating more misrepresentations

On page 227 (top) of his paper, Beyea quotes Neel's 1994 autobiography with Neel very positively reflecting

Calabrese overlooks contradictory findings in a paper he cites in 4 of his papers against Hermann Muller. In 2012, Liane Russell reported that the dose-rate effect in their earlier research was confined to large lesions. Small lesions were fully linear, so that there would be no dose-rate effect at low doses (Russell and Hunsicker 2012). This suggests a 2-hit model; repair not necessary.

This is a distraction, having nothing to do with charges of misconduct. It was also a dose-rate effect that did not continue below 300 R. And see above. Moreover, the study came after the 1956 report.

Diversion:
No, I used the Neel quote, which I included in its entirety, to show that Neel did not think his data was ready in 1956 to play other than a minor role. I said: "Neel himself felt that the 1956 report did justice to his data. As Neel indicated in his autobiography (p. 321), it was proper in 1960 and thus 1956 for the human data to play a minor role: Calabrese changes the subject to the mouse data, ignoring the implications for why the 1956 committee did not use the Neel A-bomb data in a major way.

Calabrese is shifting attention away from 1956.. Calabrese in Episode 19 as in other places, seems to think that the idea of repair meant a dose-rate threshold. It did not and does not today. Calabrese says in this episode, "...because it sounds to me that the Russell data basically said this harm is not cumulative." This is a serious misinterpretation. The dose rate dependence stopped below 300 R..Side note: Russell does not claim repair by 1958; he was being careful. His wife in 2012 found that the mutations had two components, one without dose-rate effect at low doses, undercutting the necessity of assuming repair.

Yes, this is what autobiographies are all about.

This is distraction. This is about the Muller-Neel disputes. Neel ended up with a four- to five-fold reduction in risk, not a threshold. Calabrese is talking here about evidence in 1994, not 1956.

This has nothing to do with threshold and linearity. Distraction.

Distraction. Whether or not the numbers were higher or lower still rejects a threshold. It is a debate about the slope of the LNT.

Calabrese can't help misreading the numbers in the committee report, which embellishes his claim. The range was 5 to 150, with 30 to 80 as the most reasonable, not 5 to 50.

Numbers change over time. No misconduct. Moreover, this is the doubling dose, not the total number of human mutations induced by radiation, which is not calculated today or in 1956 from Neel's or other limited A-bomb genetic data.

Not true, as I argue in Part 2. Conceptual error on Calabrese's part.

on what a good job the 1956 BEAR Genetics Panel did and that the Report has stood the test of time with no significant criticisms. At least this is how Neel remembered it. Now if only Beyea and Neel had read the same Neel autobiography more closely. In Chapter 12 of that autobiography on spontaneous mutations, Neel revealed that the background mutation rate was much higher than leaders like Muller believed back in the middle decades of the last century. As Neel writes on page 227 **"A genetic generation ago, when mutation was envisioned as a much rarer event than the evidence now suggests, H.J. Muller envisioning the human species as delicately balanced as regards its ability to cope with (i.e., eliminate) deleterious mutations, could adopt an extreme viewpoint concerning the genetic risks of increased exposures to radiation. But if the species is already coping with substantial 'spontaneous' mutations — to be sure, in ways not well understood — this alters the perspective on the risks of radiation."** Thus, Neel was clearly stating that Muller greatly overestimated the relative risk of background-radiation-induced mutation to spontaneous causes.

In the next chapter of the Neel autobiography, on page 243, Neel writes in the 30 or so years since the 1956 BEAR I Genetics Report, the doubling dose for spontaneous mutation has been revised to values now **"...about four times higher for humans than the 'official' rate, no small revision."** Based on follow-up research by Neel and a colleague, Neel noted **"that when all the data available were analyzed (and all data sets given equal weight), including some results that had been looked at askance because they didn't reveal much of an effect, the average doubling dose for acute radiation was 1.35 Gy. Applying the dose conversion factor 3 mentioned earlier [i.e., from the Russell dose rate data] to extrapolate to the doubling dose of low-level chronic radiation, we obtained a value of 4.1 Gy (i.e., 410 rads), in unexpectedly good agreement with the conclusions from the human data"** (page 244). This value compares with the range of 5-50 rads that was debated by the BEAR I Genetics Panel in their 1956 discussions. What Neel reveals is that the doubling dose estimates of the BEAR I Panel were off by a lot, about a factor of 10.

In light of this development, one has to reflect on the Neel comment that there were no significant criticisms. Yet there is Neel saying that the 1956 Panel overestimated the risks by about an order of magnitude. Also, this book was published in 1994, two years before the DOE made the Russells correct their control group data, which was too high by 120%. When such a correction was applied to the BEAR I estimates, there was no significant treatment effect at low doses, thus fully invalidating their mutation risk estimates. These comments show

that Beyea was incorrect in using Neel to claim that the BEAR I Genetics Panel did a great job that has withstood the test of time. Even Neel's comments on doubling dose contradict this, and Beyea should have reported this contradiction. Furthermore, Beyea also failed to report the criticism of the DOE just 2 y after Neel's autobiography book was published. The entire DOE story was given in the HPS documentary. Beyea also failed to report that the President of the NAS Detlev Bronk refused to share the technical reports and other work products of the BEAR I Genetics Panel with the scientific community, with most of the reports using the fraudulent data of Russell for their estimates and also hiding the fact that the highly divergent estimates of Demerec and Wright were excluded (Calabrese & Selby 2025), thereby hiding the large degree of uncertainty and variability within the Panel. This action of Bronk would have further prevented possible criticism of the BEAR I Genetics Panel report.

Distraction. I say they did not commit scientific misconduct. Their genetic model is still used today, but reduced in strength.

As I argue in Part 2, Calabrese has misrepresented the criticism of the DOE.

The academy never shares such unpublished committee materials.

As I argue in Part 2, the Russell data used in this context was fine. Calabrese and Selby made errors.

*Demerec and Wrights estimates were not of **total mutants**, which Calabrese will not face/*

Beyea misrepresents Neel's view of the BEAR I Genetics Panel risk estimates

In his Question 2 section, Beyea portrays the views of James Neel largely based on Neel's (1994) reflective autobiography. Beyea summarized the elder Neel as being pleased with the BEAR I Genetics Panel report for its clarity and that the report did "justice" to the data. Thus, according to Beyea, Neel was pleased with the outcome of that Panel's activities. However, "the real" Neel of the BEAR I Genetics Panel had a different view than what was "remembered" by the elder Neel, then about four decades later. Exactly how pleased was the "1956" James Neel with how the Panel handled the data? Let us take a look.

It is critical to note that Neel refused the request of Warren Weaver, made on 6 February 1956, to the 12 geneticists on the Panel to estimate the genetic effects of 10 R (0.1 Gy) to the human population over 1 and 10 generations due to the excessive uncertainties of any estimates. On 8 March 1956, Neel wrote to Weaver (Neel 1956d) after seeing the estimates of the nine geneticists who submitted estimates. Neel wrote: "I have been going over with great interest the estimates of the various members of the committee as they have been coming in. I still remain an unreconstructed rebel with respect to the question of whether we are in a position to make meaningful quantitative estimates concerning the genetic impact of radiation on human populations. I seem at this point to be completely overruled, but I shall go down with flags flying and guns booming to the last."

Neel's views were incorporated into the report under the category of those who did not think the challenge calculations were meaningful due to difficulties in going from animals to humans. Thus, Neel's views expressed in letters to Weaver prior to the final report had influence on the report language, as is typical in NAS reports. Neel signed off on this and other language about uncertainty.

The report states that the (illustrative) calculations are not meaningful to some geneticists (e.g. Neel). That is all you get when "completely overruled" in a committee.

On 16 March 1956, Neel again writes to Weaver (Neel 1956e) telling him that he has shared his major ABCC report with the British counterpart study committee, which had spent a great deal of time going over

Here Calabrese has a long section about James Neel involving illustrative calculations estimating damage per unit exposure of the population. Neel did not think those calculations were meaningful and this opinion was expressed in the report as "Second, this kind of estimate is not a meaningful one to certain geneticists...Also they recognize that there is a risk involved in extrapolating from mouse and Drosophila data to the human case." With these caveats in the report, one of several needed to keep the peace among committee members, there was nothing for Neel to object to, which explains his high praise in his autobiography.

Most importantly, these calculations have nothing to do with LNT, which Neel supported, although recognizing that it could be wrong at very low doses. This section by Calabrese is a distraction from both the LNT and my charges of overlooking key text. Furthermore, if the geneticists on the panel were willing to change views about the relevance of animal data to humans as he claims, they could have adopted Neel's views on the state of the art, making a stronger case for more research. They certainly would not have signed off on a radiation standard based on genetic considerations, which they did.

Neel, along with some other unnamed geneticists, is indeed dissociated in the final report from the quantitative risk estimates, as I have quoted. Calabrese cannot see the disclaimers in the report.

Distraction. There is no scientific misconduct if other committee members disagree with Neel on the adequacy of numbers His position is given in the report, included with those geneticists who do not think the calculations meaningful, as I have quoted many times in this response to rebuttals..

the first draft of his report on the Japanese experience. He indicated that the British were now very familiar with "many reservations which I had when it comes to the matter of calculating genetic damage from a given dose of irradiation at the present time."He noted that his thinking was much closer to the British committee than his own BEAR I Genetics Panel. In that same 16 March 1956 letter, Neel claimed that any possible convergences of Panel estimates were highly biased due to the actions of James Crow, who tried to force the geneticists to use similar estimates for gene number, mutation rates and other parameters for mice and fruit flies, creating likely false agreements.

One month later, on 17 April 1956, Neel again writes to Weaver on the same issue (Neel 1956f). Neel writes: "…in my role of the loyal opposition, there is at least one point that I would like to pound away at. In your memorandum of April 9, you say the geneticists do not escape their social duties by standing mute, for that decision leads to consequences. I hold that sometimes it takes more courage to stand mute and not be swept along with the crowd. In this case, I honestly believe that our calculations are tenuous to a degree where they may mislead rather than instruct. The geneticist has social responsibilities, but he also has responsibilities as a scientist. One is that in the area as critical as this one is, he must be aware of letting his conjectures get too far in advance of the facts. It is to me an exceedingly tenable position, having stated the general genetic argument, to say flatly that we know so little about the quantitative aspects of the problem that as good scientists we can only recommend that a major effort be thrown in that direction." Neel then praises part of the report by stating: "…for myself I would like to give a hearty endorsement to the part of our report that states the problem."

Neel therefore is quite clear where he stands on the challenge that Weaver gave to the geneticists and Neel's belief that the science, at that time, was not mature enough to provide adequate guidance. Of great importance is that Neel then writes: "I would like to be rather specifically dissociated from those parts of our report which attempt a quantitative treatment of the genetic risks of irradiation, in terms of the numbers of mutations produced, in which give specific dosage recommendations as regards 'permissible' levels of irradiation for populations of workers in Atomic Energy installations."

In the 12-13 May 1956 Agenda for the Panel meeting in New York City, Section 3 addresses the issue raised by Neel in his 17 April 1956 letter (Neel 1956f). The Agenda states the following: "Dr. Neel considers that the quantitative estimates…are not sufficiently definitive or sufficiently firmly justified to be useful. He therefore wishes to dissociate himself from these particular portions of the report."

The above series of letter quotes and now the Panel Agenda for 12-13 May 1956 show that Neel was consistent in his view that the science did not support providing reliable estimates of radiation-induced genetic risks. So strongly did he feel about this issue that he refused to participate, wrote about his views several times to Weaver, and demanded that his name not be associated with those sections of the report. This is a strikingly clear and consistent view. It is a perspective that also challenges the information provided by Beyea who relied upon the 1994 autobiography of Neel and his then 40-y-old memory of the Panel activities. The gold standard is what was written as shown here. It is more reliable than a 40-y-old memory. Beyea had an obligation to share with the reader the full story on the Neel views. However, again, by failing to discuss primary sources, Beyea ends up providing an unreliable perspective to the readership. This activity of Beyea is common and distorts the historical and scientific reality.

Distraction. Neel had a different view, one that was included in the report.

I relied on the actual wording in the 1956 report that used Neel's data as best it could be at the time. The 1994 autobiography showed that Neel agreed with this being a proper use of his data in the 1956 time period. I relied on the wording in the 1956 report to see that Neel's negative views on quantitative risk estimates and general uncertainty were incorporated.

BEAR 1 and the fraudulent derivation of LNT: Ignored by Beyea

In addition to his numerous errors of commission, Beyea likewise makes important errors of omission. For example, in the case of the US NAS BEAR 1 Genetics Panel, in 1995, the US Department of Energy (DOE) forced William and Liane Russell to correct major errors of underreporting cluster mutations in their control group over their decades of research based on a major investigation (Russell and Russell 1996, 1997). This resulted in an adjusted/corrected 120% increase in control group values, showing how massive the Russells' error was. Of considerable importance is that many BEAR 1 Genetics Panel members used the flawed data of the Russells in their genetic risk estimates, a point that was emphasized by Selby (2020), a paper not cited by Beyea. If the BEAR 1 Genetics Panel had used the correct data of the Russell study, the radiation-induced mutation risk estimates would not have been statistically significant; that is, there was no treatment effect for males and females, supporting a threshold and not an LNT model that all the BEAR I Panel members were compelled to assume/use (Calabrese & Selby 2025). If Russell had not provided the Panel with the incorrect control group data or if the Panel had properly questioned Russell, as Selby would later do in the mid-1990s, the 1956 report of the Panel would have been profoundly different. This fact was not mentioned by Beyea, yet it was well known. Calabrese (2017b and 2017c) had shown that Russell's uncorrected data were used by BEIR 1 (1972) for hereditary risks as well. Beyea also failed to report that William Russell, a panel member, provided the flawed data to the Panel and used it to attempt to derive his own risk assessment estimates. The

A factor of 2.2 is massive? Hardly. The committee gave a range of 5 to 150 or a factor of 30 for the doubling dose. A factor of 2.2 would at most have required a clarifying sentence in the report. What Calabrese gets wrong is the effect on the rate of mutations induced by radiation, which does not change at all, as I argue in Part 2. No change to the illustrative calculations made by almost all of those accepting the total mutant challenge who used Russell's 1951 induction data. Use of doubling dose would produce only a modest factor of 2.2 difference .

Wrong on all fronts: see Part 2. There is no statistically significant threshold using Calabrese's or Calabrese and Selby's incorrect changes to data and no change at all if their conceptual errors about background rates are corrected.

Not a fact. Wrong. As I argue in Part 2, the application of Russell's induced-mutation data for risk assessment was correct. Also, background mouse data was not used in 1972 report.

Calabrese is just wrong, because he refuses to take into account that the challenge estimates were for total mutants. As I show in the introduction to this response to rebuttals, both Jolly and Beatty were correct, not Calabrese.

So, why not look at how Weaver handled the uncertainty expressed by committee members, including James Crow? He was not deaf. I have quoted earlier the extraordinary language about how uncertain the calculations were, although sobering.

*Wright refused to calculate **total** mutations out to all generations, removing him from the total mutants challenge.*

*Demerec reported only visible damage in the first generation. He did not compute **total** mutants, which was a restriction on the challenge calculations placed in the report.*

Distraction. The uncertainty was described qualitatively in the report, accommodating the full views of the committee, e.g, "not really satisfactory to any geneticist." This includes Sturtevant.

entire episode discredits all the efforts of Crow and the Panel to estimate mutational risk effects. It also discredits the Tables of damage shown by Crow, the historians of science, Jolly and Beatty, and others. In summary, the initial data were flawed, and that error continues to propagate to the present time, thereby propping up a false LNT narrative in US regulations and policies.

Failure to share this story with the readership is a major failing of the Beyea paper, and it misled the readership on this critical issue. His article provides unreliable and incorrect information on foundational issues.

Bayea also misinformed the readership about the extensive and unacceptable uncertainty and variability of the geneticist mutation risk estimates even when using the flawed data provided by Russell. Many memos James Crow sent to Warren Weaver (which my work has documented on this topic) detail the concerns of Crow (see the next section for the Crow memo assessment). Secondly, I made my uncertainty/variability estimates based on the incorrect data from Russell. A comparison of the mutation estimates for the F1 generation was provided for the nine participating geneticists, including the estimates of Demerec and Wright, based on a composite assessment by Crow (12 March 1956), thereby having the same criteria for a fair direct comparison. This direct comparison was provided by Calabrese (2015b) in the Supplementary section, a reference not cited by Beyea. In addition, the Wright estimates ranked among the lowest of those listed by Crow in his three categories of damage. In the case of Demerec, he reported F1 damage only; it was by far lower than even Wright's estimate. The point here is that Beyea is incorrect again with his characterizations, which are also framed within the context of using the incorrect data of Russell. Beyea could have arrived at a similar conclusion had he used the primary literature or reviewed the Calabrese (2015b) paper.

Beyea tries historical reconstruction lacking primary documents: overwhelming evidence supports scientific misconduct by the BEAR 1 Genetics Panel: Beyea wrong once again

Of considerable importance is that the James Neel insistence that current science was not adequate to provide reliable risk estimates of radiation-induced genetic damage was an amazingly accurate predictor of the vast uncertainties in estimates and variations that were provided by nine BEAR I Genetics Panel geneticists. Alfred Sturtevant, a geneticist member of the Panel wrote to Weaver on 20 February 1956 expressing extreme frustration with the assignment (Sturtevant 1956). Sturtevant wrote: "After going through these calculations I come out with a feeling that they are rather futile. At almost every step it has been necessary to make a guess, often

with little to go on, and with no real basis for setting limits within which the true value probably lies." What Sturtevant was expressing here was the phrase, "garbage in, garbage out." He admitted that he could not do a professional job, let alone offer the country and entire world legitimate high-level guidance. The world community was hoping that the Panel could provide this. However, it was quite evident that it could not deliver. What would be shared with the world was largely a professional mirage that misled everyone. The fact that this effort was undertaken and published was worse than a joke. It was a high-level failure of their ethical responsibilities. Yet Sturtevant and others would keep their massive ignorance and uncertainties all in-house, protected and quiet.

Weaver saw the Sturtevant letter about his horrific uncertainty and yet wrote in *Science* (page 1163, right column): "Each thus said, in effect: 'I feel reasonably confident that the true value is greater than my minimum estimate and less than my maximum.' My best judgment, as stated in a single figure, is what I have labeled the most probable estimate." The statement of Weaver was terribly dishonest, especially with Sturtevant writing "...all that I really feel confident is the sign of the effect…," thereby directly contradicting the statement in *Science* by Weaver, a fact that Beyea was unable to know and acknowledge because he only uses secondary sources.

With such independent perspectives by the most prestigious people in the field, such as Neel and Sturtevant, there is little surprise that the estimates that Crow received from the nine participating geneticists were all over the place, a situation such as "the blind leading the blind." Being a mathematician and not a geneticist, Weaver had no idea what he was demanding when he created this mess, forcing a crisis upon Crow.

This massive individual geneticist uncertainty and lack of agreement amongst the geneticist experts created a serious concern with James Crow and would lead to a troubling professional and scientific coverup. Crow informed Weaver via several memos that if the scientific community and public became aware of such uncertainties, then none of the potential Panel recommendations, such as those dealing with public health and occupational/industrial health workplace practices, would likely have impact and be adopted. All this could easily have been predicted had Weaver taken seriously Neel's longstanding experience. Within 5 d of being assigned the task of organizing the risk estimates of the nine geneticists, Crow informed Weaver of their predicament. On 7 March 1956, Crow wrote to Weaver (Crow 1956a) saying that, "Upon looking at the estimates I realize…that nobody seems to have very much confidence in them." Crow then made what would be the key, yet ominous, statement: "…I shall pool these as best I can." So, what did he mean by this?

These views of Sturtevant are accommodated in the report. "Not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known. " At p. 1163.

The uncertain views of committee members about numerical calculations are expressed in the report, as is typical in NAS reports. Calabrese overlooks all of the caveats placed in the report. See footnote 23 in the Introduction, which lists 7 of them. For example: "Concerning these estimates of total number of mutants, three things should be said. First, they are clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known. " At P. 1163.

Calabrese quotes a letter before the final report is published, before all of the caveats are written in. Sturtevant found the caveatst sufficient for him to sign on to the report. Calabrese ignores chronology to accuse Sturtevant of dishonesty. Also, the sign of the effect is what all agreed to, along with no threshold, and what Weaver emphasized in the final report.

This claim of a cover up is one of Calabrese's failures. He refuses to see the disclaimers put into the report to handle the uncertainty felt by some members and he refuses to read the total mutant restriction put on the challenge, which falsifies his claim.

Weaver knew how to handle member's concerns about uncertainty, namely, to embrace it, acknowledge it. Weaver's overall conclusions are modest: "So that the final thing that should be said is that in spite of all the difficulties and complications and ranges in numerical estimates, the result is nevertheless very sobering." .

And "This calculation is admittedly somewhat complicated and disappointingly vague. It is, to some geneticists, not a very meaningful way of looking at the problem. To others it adds up to something at least reasonably clear, and in any event very serious."

Calabrese is repeating his old material, while refusing to deal directly with my allegations of overlooking key text in the report itself. The report separates the calculations into groups that Calabrese somehow can't recognize or face up to.

Calabrese makes up his own stories, putting himself in people's heads. As if he is a historical novelist. This is not science.

Not on point. Demerect did not calculate ***total*** *mutants, so his estimate does not belong with the group of six that were estimates of total mutants.*

This is not relevant. Wright as he says in correspondence to Weaver would not consider estimating total mutations, so his estimate does not belong with the six. Yes, his methods were greatly different. It was not a mathematical disagreement, but a philosophical one as to what mutations should be considered, as I have quoted from his correspondence.

Distraction. They were considering different mutation outcomes

..

Consider first the assignment/responsibilities that Weaver had given Crow

In a 2 March 1956 brief summary (Weaver 1956b) of the March 1 meeting, Weaver indicated that "Jim Crow was made the chairman of a subcommittee (he can commandeer help on this if he wishes) to go through all the damage estimates, compare them, and display assumptions, methods, input, and results in some sort of chart or graphic form." This description was very specific, and it did not indicate that Crow was authorized to exclude individual contributions nor to make judgments as to which estimates were the most/least credible or what should be shared with the scientific community or general public. Crow's role as subcommittee Chair was important but limited to organizing and integrating the submitted estimates in a coherent manner so that the entire Panel could intelligibly, efficiently, and objectively view the submitted estimates. As we shall see, Crow went beyond what Chairman Weaver had authorized, making decisions concerning the estimates to keep and what to exclude, without any Panel discussions, apparent consensus, and votes. Now some 5 d later, Crow wrote to Weaver on 12 March 1956 (Crow 1956b), informing him that he had excluded the estimates of Demerec because they were based on different assumptions than those using fruit fly and mouse models that led to, "...a greatly different value than the others obtained." That is, the estimates of Demerec were far lower than those using the fruit fly and mouse models. In the case of Wright, his assessment counted, "...mutations causing conspicuous effects in postnatal life." In point of fact, Crow misrepresented what Wright had actually done. Crow stated that the risk estimate report of Wright was based on, "counting mutations causing conspicuous effects in postnatal life." However, the Wright document states that his analysis included the "conspicuous detrimental effects on viability or fecundity." Thus, the effects estimated by the Wright analyses would not have been restricted to those occurring in postnatal life, thereby contradicting the statement of Crow. This would prove to be a significant error by Crow, affecting the outcome of the Panel by Crow's decision to exclude the Wright estimates. Crow also noted that Wright's methods were "greatly different," but whatever that meant is not clear. However, Wright was considered the most outstanding mathematical talent on the Panel, being far beyond those considered excellent. It is doubtful that Crow would challenge any detailed evaluation of Wright. As in the case of Demerec, the risk estimates provided by Wright based on human data were far lower than those derived from the fruit fly and mouse estimates.

On 24 March 1956, it appears that Crow knew he was on scientific thin ice, writing to Weaver (Crow 1956c) that, "This may be too late to be helpful, but for what

they are worth here are my comments. I have considerable doubt about the wisdom of the set of estimates in section XII. The limits are too wide, so maybe no estimate is better." He went on to propose an alternative based on a recommendation from Joshua Lederberg (i.e., a 1958 Nobel Prize recipient along with panel member George Beadle). Lederberg recommended that they base the estimates upon mouse data, dropping even the fruit flies. He claimed that it could give very precise estimates even if surrounded by considerable uncertainty. Lederberg noted that "much of our information in medicine (for example, safe concentrations of industrial poisons and insecticides) comes from extrapolation from experimental animals, extrapolations subject in many cases to greater uncertainties than we are dealing with in our study." In fact, it was too late as he continued to move the quantitative estimate agenda forward. In that same letter, Crow informed Weaver, "I think you give the impression that the seven estimates were independent, whereas in fact six of them made almost identical assumptions." In fact, based on the 24 March memo, it appeared that Neel had an impact on Crow.

In his 29 March 1956 letter to Weaver (Crow 1956d), Crow reveals that he had made a decision that the estimates of the Panel will be based on the data from fruit flies and mice only! Despite his unauthorized and therefore unjust exclusion of the bacterial and human data, Crow was still greatly concerned with the dominating and overriding political issue, stating that: "The limits presented on our estimates of genetic damage are so wide that the reader will, I believe, not have any confidence in them at all. I suggest one of two things: a) omit the estimates entirely, or b) give a single best estimate of the number of mutations, or a narrow range of estimates, based on direct extrapolation from mouse and Drosophila. We then state that those are based on mouse data and let the reader add his own safety factor."

It is clear from the statements of Crow that even with the improper inclusion of only the fruit fly and mouse estimates, their agenda was still in major trouble. If they had included the bacterial and human estimates, it would have gone beyond having no confidence in their estimates at all, the equivalent of professional ridicule for this very distinguished Panel. In fact, the Panel would have been exposed as not very expert at all, a group that was acting outside its professional wheelhouse, a blow to the prestige of the National Academy of Sciences as well, perhaps even more so.

On 21 May 1956, nearly 3 mo after Crow first raised his concerns to Weaver on 7 March 1956 about massive uncertainty among the BEAR I Geneticists with respect to their genetic damage/mutation risk estimates he still cannot shake this issue. In his letter to Weaver, Crow writes:

Distraction.

Distraction. The estimates were of different types, only some of which were total mutants.

Weaver took a different approach than did Crow to handle lack of confidence. He embraced it,, as I have written above. Calabrese overlooks the seven caveats about the calculations that appear in the report.

Calabrese makes up conspiratorial agendas for the committee. The final report gives their goal and recommendations.

Calabrese makes up stories here, ones based on false premises, as well.

Distraction. Weaver handles this uncertainty in the report generically, saying not really satisfactory to any geneticists. "Too much has to be assumed, too little is dependably known."

Calabrese makes up stories again, acting as a historical novelist taking liberties with the history.

More story telling, leading to claims of dishonesty.

Calabrese ignores all my arguments about geometric standard deviations, which I first made in 2017 in detail. See item 40 in my annotations to Cardarelli's rebuttal. Math is not one of Calabrese's strengths. 100 fold is still a huge uncertainty.

The report acknowledges the full uncertainty publicly. Calabrese should acknowledge the quotes about overall uncertainty.

NAS never gives out the deliberations of its volunteer committees for at least 25 to 50 years.

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Documented by Calabrese? Not at all. Calabrese has misled his readers.

The Kaufmann estimates (by Moser) did not include an uncertainty range, which was the second requirement given in the challenge as stated in the report. It thus failed to meet the qualifications. Moreover, its inclusion would not have changed the geometric mean by much. In Calabrese's latest paper, he and Selby contradict what he says here, stating for Moser that: "...thus his calculation of the Most Probable Value shown in Tables 1, 3 and 4 is identical to that of Glass."

"I don't like to include that table on page 22; in fact, I thought we had agreed not to do this. The wide limits, and the wide differences between the different estimates of the limits made by the different persons, stem largely from differences of opinion as to the uncertainty in going from mouse and Drosophila to man…. Once again, I urge that we not include the table at all."

Crow and Weaver quickly found themselves between a so-called rock and a hard place, with no apparent escape. Acting in a bizarre manner, the only way out of this dilemma was for Crow to constrain the estimates to those based only on fruit flies and mice. However, Crow and the Panel would still have the problem of the remaining uncertainty being too great. How could he and the Panel find a way around this remaining major scientific roadblock. It was actually easy, but one had to be dishonest to achieve it.

Here is how Crow and the Panel solved their problem. They got around this remaining excessive uncertainty and variability issue by now claiming that the uncertainty was 100-fold, something they must have felt would work well with the scientific community, even though it was highly variable with two of the six "expert panelists" expressing uncertainty of 2,000-fold. The Panel was simply overwhelmed with uncertainty but did not want to acknowledge it publicly. The Panel then refused to share their findings, that is, how they derived this value, when members within the scientific community challenged the Panel. It soon became a career-threatening drama for all the members of the often-cantankerous Panel. They even got NAS President Bronk to participate in this scientific fraud by convincing him to deny a request from outside scientists to have access to the work product of the Panel.

This secret was hidden for 70 y, until I discovered the scientific coverup. This story has been documented in my publications and summarized in the documentary. By removal of the estimates of Demerec and Wright, it became clear that the overall variability in the group estimates would be markedly reduced, suggesting a way to deal with the high variability issue. However, the story was still not complete as there remained the Kaufmann estimate for human responses. His estimates were the third from the lowest, just above those of Demerec and Wright. So, Crow easily removed the Kaufmann estimates, now bringing the total usable estimates to six from the original nine, even though they were as legitimate as any that remained. Within this context, it is important to see what the conclusion of the Kauffman Report stated: "Calculations show that under the defined conditions the visible genetic damage resulting from chronic exposure to 10 r per generation is small in comparison with that of spontaneous origin." This general conclusion matched that offered by Demerec as well, yet it would remain

hidden by the actions of Crow and the Panel. In fact, this was most likely the reason why this report was dropped by Crow.

When the Panel published their technical report in *Science* journal in June 1956, the Panel included information ONLY for the results of the first generation (F1 generation). The *Science* paper did not provide any information on the biological models and methods used. That is it. Even though estimates were requested by Weaver for "at least" three areas (first generation with one radiation dose, 10 generations with one radiation dose, and 10 generations with 10 radiation doses/one per generation), the only information EVER provided to the scientific community by the Panel was the estimated response of a single generation (i.e., F1 generation) as summarized in the *Science* publication. In fact, this was enough to get their point and concerns across with no need to further complicate the issue. This F1 estimation information was also provided in the reports of Demerec, Wright, and Kaufmann—but Weaver, Crow, and the remaining Panel members prevented their estimates from being shared in any fashion. Furthermore, even though the *Science* journal paper indicates that six geneticists contributed the estimates, no specific information was provided that identified who provided the estimates or what any specific Panel member estimates were. The report of the Panel was largely derived by deliberate scientific misconduct and with an organized and firm cover up, all passed up to the President of the National Academy of Sciences. The cover-up also involved the journal *Science* since Bentley Glass, a member of the Panel, was also a senior editor at *Science* at the time. It became quite the scientific cabal.

Beyea tries to dispute this story. In his approach Beyea never obtains the primary information, relying on secondary sources. In the case of the Panel, this approach of Beyea is inadequate. For example, I had to make an extraordinary effort to get complete files on all memos, letters, and draft reports of the entire set of all the players in this risk assessment drama. I did this because I was concerned that no single person's file would be complete. So, I purchased the files from all the participants and organizations and painstakingly pieced them together to see how much overlap and completeness there was. Then I worked to create a master file that would be as complete as possible, with thousands of pages and hundreds of documents. These are documents that would reveal the story that I have published and briefly summarized here. The "hidden" story was hidden for a reason, and the literature is hard to identify, obtain, and integrate. Of critical importance is that Beyea tries to create his version of the story without obtaining the original documentation, where the facts can be found. Having undertaken the massive effort of reconstruction and then privately sharing

Calabrese makes up reasons to suit his story. In fact, there is a straightforward reason why Moser's number, identical to Glass's, was removed from the challenge: no uncertainty values.

In this section, Calabrese shows how wrong he is.

Calabrese is wrong. The total mutant calculations included recessives, which do not show up in the first generation. Only tangible mutations show up in the first generation. An estimate for those was given separately in the report, as I wrote in both 2025 and my 2017 paper, which Calabrese never reviewed.

Demerec and Wright largely excluded recessives, which don't show as tangibles until far into the future, as acknowledged in the 1956 report..

This is Calabrese's conspiracy story, and nothing can shake him from his overlooking of text. Readers must do the looking for him and decide.

To confound Calabrese's interpretations, I rely on the wording in the 1956 genetics report, which is a primary report, not a secondary report. Calabrese ignores chronological order.

This is Calabrese telling us how hard he worked to request documents from libraries and NAS, without giving any credit for the science historians, like Jolly, Creager and others who have gone before him, listing the locations and identifications from where they got their material. I did rely heretofore in part on the assessment of correspondence by trained historians. Calabrese challenges them (trashing Jolly), but when I checked correspondence, I found they were correct and he was wrong. I now show that even with correspondence, Calabrese misses contradictory information.

See previous sentence. In addition, I rely on the early scientific literature and other reports, as well as transcripts, which are all primary documents.

Calabrese shared his story with several interested senior scientists, without telling us who they were, something which I have done. Why not historians of science as I have done? Why not a statistician as I have done? Calabrese claims he learned the craft of science historians by himself, without needing any training or collaboration. We are not told how he did this or what curriculum was involved. In tracking down his work, I find that he does not show any idea as to how to produce a balanced interpretation. He had a bad teacher of history: himself.

In the introduction, I show that Beatty was correct and Calabrese was wrong about Wright. I did so pointedly by using the original correspondence of Wright to Weaver, to which Beatty would surely have had access. Beatty did not respond to Calabrese about this, but I have done so, based on correspondence that I collected in 2017. I do not like to rely on my own assessment of correspondence, not vetted by historians, but I do so for this reply to rebuttals.

Again, Calabrese misses contradictory information. Calabrese apparently did not read the Wright correspondence or forgot it. As for Demerec, Calabrese's latest paper supports what I have said. His Table 1 shows no estimate for Demerec for "Task # 1 on total damage." Wright refused to participate in computing total damages, to which he objected, computing a lesser type of damage, including weights for more severe tangible damage. Note that Calabrese's Table 1 incorrectly lists Wright as giving total damage.

We must have read different letters, or Calabrese engaged again in selective stakeholder perception. I discuss Wright's letters to Weaver in the introduction. If Calabrese thinks that he can put his own spin on correspondence, because no one will check him, he is wrong.

my developing story for critical feedback with several interested senior scientists with a profound interest in the topic prior to submitting my work for publication, I can only view the efforts of Beyea in this area as grossly inadequate, within the context of thinking that he could develop an historically based explanation without examining the primary source documents. While Beyea may offer his speculative hypotheses and criticisms, they are based on grossly inadequate foundations that are not supported in the factual record.

Several specific examples of what Beyea employed to support his hypothesis that there was no scientific misconduct by the BEAR I Genetics Panel will now be assessed. Beyea cites one source as the basis of his hypothesis, that being the paper by the historian of science, John Beatty (2006). Beatty stated that Wright, Neel, and Demerec "...explicitly refused to contribute a number to the final report." We know that this is the case for James Neel, as it has been thoroughly documented with his story being deeply integrated into the present assessment. However, in contrast to the very explicit and documented criticism of the Weaver assignment by Neel (i.e., refusing to provide detailed estimates), neither Wright nor Demerec acted in the manner of Neel. They contributed highly detailed assessments that must have taken considerable time to produce and finalize and provided them to Weaver within several weeks of the given assignment. Careful reading of their cover letters and reports provides no evidence that supports the undocumented assertion of Beatty about the so-called, "explicit refusals" of Wright and Demerec.[3] No evidence supporting the statement was given in the detailed dissertation of Jolly (2003), which had a strong focus on the BEAR I Genetics Panel. My repeated requests to Beatty for clarification of this issue via emails (12 February 2025 and 18 February 2025) and recorded telephone messages received no response. In addition, multiple detailed letters/memos from Wright and Demerec to Weaver sent from March to May, that is, after the estimates were provided, offer no evidence of the "explicit refusals" or refusals of any kind. In the case of Wright, he became embroiled in an acrimonious debate with Muller over Muller's assertation that mutations should be considered within the framework of a "genetic death" concept. Wright's opposition to

[3]In his discussion, Beyea states, "Calabrese is apparently unaware that one of the excluded three committee members (Sewall Wright) did not want his estimate used (Beatty 2006) and the other two did not include all expected mutations in their calculations, so their reports were reported separately in the report." Beyea was "apparently unaware" that I sent an email to Beatty on 24 February 2016 stating: "On page 63, it is stated that several distinguished geneticists, including Wright, Neel, and Milislav Demerec, explicitly refused to contribute a number to the final report. I am aware of the Neel refusal but not of Wright and Demerec. Could you provide me with the source that supports that statement?" Beatty acknowledged this email, noted that his papers were in storage and would provide a response/answer to me. He never provided an answer to this request. The point of this story is to highlight that Beyea continues to make mistakes and offer unreliable speculations and is shown to be often wrong. In this case, his style is to cast doubt about my professionalism and knowledge to the reader.

this interpretation started mildly in writing as early as 17 April 1956 in a letter to Weaver (Wright 1956).

The issue soon became very inflamed with Panel member supporters on either side. Threats were made by Muller and Wright that they would not sign the final report if the other side's view on the genetic death concept predominated. Weaver diplomatically resolved this issue with the story behind the story told 40 y later by Crow (1995). This dispute was not related to the original Weaver assignment. Thus, there is no evidence that the assertion of Beyea is supported. Of importance, once again, is that Beyea depended entirely upon a single, and very limited, in scope, secondary source, when it was necessary to obtain primary documents. Beyea also stated that the Wright and Demerec findings, which they "explicitly refused to contribute to a number in the final report," were nonetheless acknowledged and cited in the Panel report, but there is no evidence that this was the case. In fact, the findings of no specific geneticist are identified in the *Science* journal publication or the Report to the Public. In addition, Beyea did not seem to understand that the only endpoint reported in the *Science* journal paper was that of the mutation estimate after one generation. This information was provided by Demerec, Wright, and Kaufmann, as documented by Crow, and therefore could not be a basis for their exclusion. In addition, the entirely documented set and sequence of the Calabrese description of the BEAR Panel activities was not challenged in the Beatty (2006) report.

Beatty (2006) also makes a misrepresentation of the factual record on the Panel activities. He stated on page 64 of his 2006 paper that, "While it is true that six of the geneticists of this committee considered the following problem, it is truer that all thirteen geneticists were asked to participate and seven declined." The statement of Beatty is in error in several ways. First, while there were 13 original geneticists on the Panel, Professor Charles Cotterman from Baylor University withdrew after the 6 February 1956 meeting due to academic obligations, leaving only 12 geneticists (Weaver 1956c). Beatty (2006) mistakenly infers that Cotterman would have affirmatively declined to offer genetic risk estimates. However, in the telegram to Weaver, Cotterman stated in capital letters: **"Under other pressures have been unable to work on project or read communications for many weeks and therefore have no suggestions stop as conditions show no prospect of change regret I must resign as useless committee member stop travel allowance and documents will be returned for redistribution."** It is clear from the Cotterman telegram that Weaver shared with Neel on 28 March 1956 that Beatty was incorrect to suggest that Cotterman remained on the Panel but refused to do the assignment.

*Yes, Wright's opposition to the "genetic death" concept is why Wright would not compute a **total mutant** estimate, which was based on the framework of genetic deaths. Calabrese again supports my argument.*

Yes, the genetic death concept was related to the original total mutants assignment. Without it, total mutants made no sense.

I depended on the assessment of correspondence by a trained historian. I was correct to do so, as I found out by rereading Wright's letters to Weaver, as shown in the Introduction and validated above by Calabrese as to the dispute between Wright and Muller. Calabrese selects his correspondence.

Wrong. A second endpoint, "tangible" mutations in the first generation, was given in the 1956 report, distinct from the total number that included recessives, which show up later, far into the future.

Here we have an example of flawed logic. Calabrese does not distinguish between the tangible mutation in the first generation and those that are passed to later generations, e.g., recessive mutations, which are only tangible in later generations. How could he not understand that? How could he not see that the 1956 report gave illustrative estimates for both?

Distraction, irrelevant.

*Beatty is talking about the **total mutant** challenge, while Calbrese is talking about lesser tasks. The seven took up the **total mutant** challenge.*

Calabrese is wrong as I have discussed above and in the introduction.

*I relied primarily on the 1956 genetics report itself, which described the challenge undertaken by six geneticists as one of **total mutants**, with uncertainty ranges. I also relied in part on the secondary sources of Beatty and Jolly, who are professional historians. Calabrese's criticism of them falls flat on checking.*

Calabrese fails to note that the recessive mutations in the first generation become tangible only in later generations. The 1956 report gives separate estimates for tangible (sometimes called visible) mutations in the first generation and those that are tangible out to the 10th generation and beyond (total mutants). The report explicitly discusses total mutants. How can Calabrese not see these words, even when he quotes them below?

I have added bold formatting to those words in the quotation that Calabrese claimed above were not in the report. Even when he quotes the passage describing the nature of the calculations, he cannot see "total number of mutants."

Distraction.

Secondly, Beatty stated that seven geneticists declined to participate and did not provide estimates, when in fact, I possess the submitted copies and cover letters of the nine participating geneticists received by Weaver. That Beatty would write in this manner suggests that he did not possess copies of these critical nine submitted reports and their cover letters at the time he published his 2006 paper on which Beyea relies. Based on the above information, the assertions of Beyea are not supported. As such, the *HPJ* reviewers and editorial leadership should have required that a higher-level professional effort be made by Beyea. Beyea persisted in his absolute reliance on secondary sources, such as the Beatty (2006) paper in the present case. The present assessment shows that the Beatty paper provided incorrect information of a highly relevant nature that has never been corrected in the record. Yet, this secondary source is the Gold Standard for Beyea rather than obtaining the primary documents himself. This assessment indicates that Beyea failed to do an appropriate professional effort, providing unreliable information.

More Beyea errors concerning the BEAR I Genetics Panel risk estimates

An important concern is that Beyea did not carefully read the BEAR I Genetics Panel 1956 *Science* journal paper. It informs the reader that Panel geneticist estimates are based on a single 10 R (0.1 Gy) exposure with effects estimates in the next or F1 generation. That is, the estimate is presented for a single (i.e., the next) generation, not the 10 generations that Beyea claims or possibly Total Damage that affects all descendants.

In the Genetics Panel 1956 *Science* paper, it was stated that *"Six of the geneticists of this committee considered the following problem: suppose the whole population of the United States received one dose of 10 roentgens of radiation to the gonads. What is the estimate of the **total number** of mutants which would be induced by this radiation dose and passed on to the next total generation of about 200 million children?"* (italics added).

Making the situation far worse is that the poorly reproduced figure in the Jolly dissertation (p. 350) that Beyea depended upon removed one name entirely from the F1 graphed estimates and so poorly reproduces three other geneticists' names such that they cannot be read under the largest computer provided magnification and then superimposing a further magnification of 20-fold via a magnifying glass. In addition, the Jolly figure failed to even describe the graphed information concerned; that is, there was not even notification of the type of genetic damage given (i.e., F1 genetic damage estimates). This poor and totally unacceptable figure reproduction by Jolly that

Beyea used prevents the reader from assessing differences in geneticist estimates and their uncertainties. Once again, the failure of Beyea to obtain primary documentation led to Beyea being incorrect in his fundamental understanding of what the estimates of damage were for each geneticist and of the Crow presentation. That Beyea would not attempt to obtain the original Crow estimates in light of the grossly inadequate Jolly reproduction raises serious questions about his methods. Beyea passes this confusion and error on throughout his entire paper. These limitations and mistakes contaminated the recent Beyea paper and affect anyone who uses or cites the earlier Beyea (2016) paper.

This situation became greatly aggravated when Beyea failed to acknowledge the subsequent primary literature memo of Crow to Weaver in which he claimed that estimates would now be based on only fruit flies and mice, leading to the dropping of the Wright estimates, which were by far the lowest of the remaining estimates, realizing that Crow had earlier dropped the estimate of Demerec based on his research with bacteria, which provided the lowest overall estimate for the F1 generation.

It is now clear that the estimates for the F1 generation were the only ones reported in the official technical BEAR 1 Genetics Panel report in *Science*, June 1956. It is also clear that Beyea relied on the flawed graph in the Jolly dissertation and did not obtain/consult the multiple memos of Crow on the issue with his inclusion and exclusion of studies that were strongly influenced by how they affected the occurrence of geneticist uncertainty and variability.

Thus, in summary, the 12 remaining geneticists of the Panel were invited to provide estimates, but only nine did. Crow initially excluded the values of Demerec because they were far lower than the other reported values. Nonetheless, the Demerec estimate was based on extensive research with bacteria in his own laboratory. That estimate was very professionally done with "data for 28 genes with regard to frequencies of spontaneous and X-ray induced visual mutations." From these studies Demerec reported the radiation dose needed to double the spontaneous mutation frequency, a critical need for the Panel, yet Crow discarded it. In fact, the report of Demerec offered its own experimental findings that were extraordinarily comprehensive. Demerec only provided estimates for the F1 generation, claiming that estimates out to 10 generations were too speculative. Crow provided the estimates of the remaining eight geneticists in his 12 March 1956 letter (Crow 1956b) for three genetic damage categories. It was clear that the estimates based on human data for Wright were consistently far lower than those provided by fruit fly and mouse data. In the next memo Crow indicates that the estimates of risk would be based on only these two species, thus dropping the human estimates (Crow 1956c).

This is so silly, because Table 5 in Calabrese's latest paper, written with Selby confirms what I have said in the past and what Beatty said about the total mutant calculations (Task 1) C&S have Sewall Wright giving estimates for Task 3, not Task 1. This is consistent with Wrights correspondence to Weaver as I have indicated.

Calabrese may mean my 2017 paper, to which he has never responded, but for isolated comments in his rebuttal.

Repetitive. I dealt with this false description earlier. Also, Table 5 of C&S discusses Wright using mice data. Wright's estimate was not of total mutants (Task 1), which is why it was not included with the other six

Misleading, as I have discussed. There are two types of estimates for the F1 generation, those that show up and those that also include those passed on to succeeding generations.

Rebutted previously. Calabrese gets lost in early correspondence and does not consult what actually appears in the final report.

Calabrese makes up his own reasons to fit his narrative, ignoring the total mutant distinction and the ambiguity of estimates for the F1 generation, without specifying whether tangible or total..

Double what? Demerec did not compute total mutations to double, as Calabrese and Selby confirm (Calabrese and Selby 2026) in their Table 5. "He noted that the mutations were all visible mutations."

Demerec's estimates for the F1 generation were not complete totals, they were for tangibles (visibles).

Wright made estimates of doubling dose limits for humans, but he used mice data to compute estimates for Task 3 mutations. My source here: (Calabrese and Selby 2026) at Table 5.

So, what happened to my 2017 paper to which Calabrese never responded? I only discussed these quantitative estimates in 2017. In any case, the reader will have to decide who got the allocation of estimates correct and who got them wrong.

More claims about my 2017 paper. Calabrese does not say how my supposed errors were supposed to have affected my 2025 paper in Health Physics. He seems to want to provide support for his idea that my papers in 2017 and most importantly 2025 depend exclusively on secondary sources.

So now, Calabrese in this section concedes that the illustrative calculations by numerous committee members were based in part on the Russell data. In Part 2, I argue that the Russell data was fine for making radiation induced estimates of genetic risk.

The discussion here by Calabrese is apparently about my 2017 paper, not the current paper in which I critique his overlooking key historical text, setting up false premises and making statistical errors.

*This all has nothing to do with my claims that Calabrese overlooked the definition of the estimate type made by six geneticists, namely **total** mutants in the first generation. In any case, Calabrese confuses agreement on central values estimated by the six, which was good as recalled by Glass (Beyea 2016a), versus the wide range in their uncertainty bands that Crow used, which was poor.*

Calabrese's attempt to assign a "Gold Standard" to the transcripts seems to me to be an attempt to escape the final language in the report. Committee members often change their ideas throughout the process and much happened in 1956 after the second meeting transcript. Chronology matters, although not to Calabrese, apparently.

More discussion of 2017. Not true. I have relied on the actual 1956 report itself to define the estimates that were included in the problem assigned to geneticists, which narrowed down the candidates to six. Calabrese refuses to face up to the total mutant restriction and the fact that Wright did not, and would not, compute total mutants.

In summary, Beyea made key mistakes in his 2016 paper that were never corrected and affected the Beyea (2024) paper in *HPJ*. The incorrect understandings of the Crow geneticists' mutational risk estimates, his misreading of the Crow graph via the use of the inadequate graph reproduction of Jolly (2003), and the failure to research the evaluations/judgments of Crow via the primary literature led to these multiple errors. These errors then led to additional data interpretation errors and strikingly incorrect criticisms of my research publications as noted in this paper. The information that Beyea provides is factually incorrect and interpreted improperly, providing information and interpretations to the reader that are not reliable. In addition, and most important, is that the entire Crow estimate exercise is scientifically meaningless since numerous Panel members based their estimates on the flawed and incorrect Russell data.

Furthermore, Beyea displays the same total reliance on secondary sources of a dissertation by Seltzer (2007) on the BEAR 1 Genetics Panel. In this case, Beyea noted that Bentley Glass recalled that the large initial disagreements among the geneticists on radiation induced genetic risks were resolved during an evening meeting, with general consensus being displayed the following day. Thus, according to the Glass recollection, the issue that I have focused on was never a real issue but quickly resolved by the geneticists of the BEAR 1 team after their nightly meeting. Since Beyea relies only upon secondary sources and since Glass was present at these meetings as a real eyewitness and participant, Beyea sided with the Glass story. However, I pointed out that the 85-y-old Glass, now with a 40-y recollection, got the story wrong, based on the Gold Standard, the meeting transcripts (Calabrese 2017b). Glass confused a meeting of the geneticists at their 20 November 1955 meeting at Princeton with the activities of their 6 February 1956 meeting in Chicago. There are transcripts for both meetings that allow one to figure out whether Glass was correct or not. This confusion of Glass is readily documented if one consults the primary transcripts to provide a type of test for the quality of the failing memory of the elder Glass. However, the key point herein is that Beyea emphasizes that when needed, he tries to find reports of professional historians of science, such as their papers and available dissertations. However, as in the case with Jolly (2003), Beyea failed to discern that Jolly's poorly reproduced Crow figure led to erroneous insights and conclusions. In the case of the Glass memory, Beyea relied upon the reporting by Seltzer (2007) that was accurate but was incorrectly interpretated by Beyea, possibly because Seltzer did not adequately cover the 5-6 February 1956 meeting in Chicago with similar transcriptional records. The major point is that Beyea's approach to research in this area is often

professionally precarious and not reliable. This approach of Beyea could never have survived a rigorous graduate student experience, but it passes the peer review of *HPJ*.

There are additional serious problems that Beyea's research approach creates. That is, I have addressed a range of issues concerning the historical foundations of cancer risk assessment. Some of these issues have been considered by historians of science but many have not been considered by them. What this means is that Beyea allows himself to address historically what I have done, if it might have been addressed by historians of science whose research he can learn of and access. Thus, his approach only addresses a very small fraction of what I have published, and for that small proportion of the topic, he used secondary sources that have misled him, been misinterpreted by him, or otherwise have not often covered precisely the same issues as I have. The bottom line is that his research method does not have the required rigor. Again, the lack of reliability of the Beyea approach was missed by the peer reviews and editorial leadership of the *HPJ*.

Beyea makes more errors concerning the BEAR 1 Genetics Panel genetic damage/mutation risk estimates. This is again caused by his total reliance on secondary sources, which in this case is the dissertation of Jolly (2003) and the paper of Beatty (2006). The new error is that Beyea failed to detect that James Crow used estimates for genetic damage for Beadle, Muller, and Sturtevant for the 10-generation/single 10 R (0.1 Gy) exposure [as shown in the Beatty paper (p.63)] based on their exact F1 generation data. That is, he transposed F1 data into a 10-generation category, thereby under reporting damage estimates by about tenfold for each of these individuals. Beyea missed this error because he did not use Crow's letter to Weaver on 12 March 1956 that graphed all panel estimates, and he failed to match these values against the technical documents of each of the panelists, again all primary documents. The mistake of Crow was not detected by Jolly or Beatty but by me. This means that the Jolly and Beatty reports are also in error and Beyea used these as his special Gold Standard, his academic bibles. The point is that Beyea had no chance to detect these important errors unless he used primary sources. By missing this error Beyea was unable to see that this "error" of Crow resulted in reducing interindividual variability and masked the magnitude of disagreement among the panelists. This is another in a very long list of examples where Beyea provides incorrect and unreliable information, and he has no idea that he is doing so because his methodology precludes the proper evaluation. Regardless, the information and interpretations of Beyea in these areas are incorrect. This was also missed by the peer review process, again showing that it acted in many ways like a non-discriminating sieve, simply allowing a massive flow of unreliable information

Here we have a standard tactic of Calabrese's, namely, to set himself up as some arbiter of academic quality and thereupon try to belittle his targets as academically inferior to him. I am honored to be in the same league, albeit in such a small way, with Edward B. Lewis.

I have confined myself, as I have stated in 2025, to defending the early researchers who he has denigrated. I have not criticized his non-historical efforts as others have. I have not waged an all out attack on Calabrese, as he has done with Muller in his opposition research. His professional work in toxicology does not allow him freedom to say whatever he wants in fields for which he is an amateur, such as science history, forensic science or statistical inference. My research method is simply to follow his footsteps and point out his errors. I have found that I cannot always replicate what he writes, which makes his historical work unreliable without checking. Not all of his work is unreliable. The problem is knowing which is and which isn't. The unreliable part seems to peak in my checking his work when it comes to claims of scientific misconduct or other ethical violation.

I did leave out words in this case in the Summary, thereby misreporting the main text, although the point was still correct, namely that there were two types of calculations presented. I should have included on p. 519 the words shown next in bold format: "... the two low estimates only covered first-generation ***tangible*** *damage, unlike the others that went out to 10 generations* ***and beyond****." I thank Calabrese for pointing out this editing problem in the Summary. I have posted the correction to the Errata. This has nothing to do with Crow's estimates.*

The report did not mask disagreement. What does the report say, ""Concerning these estimates of total number of mutants, three things should be said. First, they are clearly not really satisfactory to any geneticist. Too much has to be assumed, too little is dependably known." The fact that neither Calabrese nor Cardarelli in their enormously long rebuttals face up to this kind of uncertainty language is evidence that they have no adequate response.

generated by Beyea to be published. As noted above, even though this report has documented numerous errors and failing of the Crow genetic damage evaluation process, the entire Crow estimate exercise on which Jolly, Beatty, and Beyea based their genetic damage risk estimates and risks estimates is scientifically meaningless, since they were based on the flawed and incorrect Russell data right from the start (Calabrese & Selby 2025).

Beyea uses the data of Hanson and Heys that Muller claimed was fraudulent

Hanson and Heys published a number of papers concerning radiation-induced mutations that were used to support the LNT model. Hanson had in fact had a sabbatical in the fall of 1927 with Muller at the University of Texas. The series of papers by Hanson and Heys have been widely used as strong support for the linear position. I have commonly cited these papers in many past publications. However, several years ago I uncovered a letter from Muller to a senior academic geneticist colleague that told the story that he believed that the papers Hanson and Heys collaborated on were most likely fabricated and not reliable, and that they should never be cited. Muller never made these damaging remarks public, possibly because Hanson had become his grant manager at the Rockefeller Foundation, and he may not have wanted to affect his funding from that organization. Nonetheless, I have published a detailed assessment of the Muller criticisms of the Hanson and Heys research (Calabrese & Giordano 2023). However, Beyea (2024) continued to cite the research findings of Hanson and Heys without sharing the now revealed serious concerns of Muller. This failure of Beyea to identify these concerns and address them in his paper adds yet another reason why the information presented in his paper is unreliable.

I mentioned Hanson & Hays once: "Research standards vary over time, but replication remains the gold standard. Muller's production of mutations by xrays, for which he won the Nobel Prize in 1946, was replicated and published (Hanson and Heys 1928; Patterson1928; Weinstein 1928; Whiting 1928) within approximately 1 y of his 1927 Science publication (Muller 1927b). Muller and Hanson/Heys were competitors, especially about the experimental support for the proportionality rule. It would not be surprising for Muller not to think much of their work. Muller continued to cite Hanson & Heys years after the 1948 letter that Calabrese mentions (Muller 1957).

Calabrese says I made a deliberate misrepresentation. Here, he relies on his signature method of claiming misconduct on those with whom he disagrees. I am in good company. And, by the way, he does not actually make such a misrepresentation claim in the text of the section.

Distraction. The point was that Weinstein replicated Muller's results, regardless of the high doses. Calabrese does not dispute the replication of Muller's method for producing copious mutations.

Distraction. So, what does it matter that Whiting's study "was performed with a wasp model, while Muller used the fruit fly model"? X-ray-induced mutations were found.

Other mistakes of Beyea and deliberate misrepresentations

1. Beyea cited the papers of Whiting (1928) and Weinstein (1928) as having replicated Muller's findings. There was little point to noting this. As was the case with Muller, Weinstein used extremely high doses and dose rates. Muller's dose rate was about 100 million times background (Calabrese 2019a, 2019b). This was similar for Weinstein who followed the Muller protocol. As for the Whiting paper, no data were provided to evaluate, and it was performed with a wasp model, while Muller used the fruit fly model. There was also no reporting of the doses used or the sample sizes employed. Thus, it is not clear why this was cited or even why it was even published in the first place. Yet Beyea oddly used it as support for Muller's findings. Based on the lack of information provided in the

Whiting (1928) report, it suggests that Beyea may not yet even read the Whiting paper. Thus, using a study with no data provided and no experimental protocols on even the most basic of requirements is very troubling. Thus, it is not clear why this paper was cited. Furthermore, the Muller research produced mostly moderate to large/massive gene deletions and not the vast point mutations Muller incorrectly proclaimed in his title (i.e., Artificial Transmutation of the Gene) (Muller 1927a). These references were poorly selected by Beyea, making the Beyea argument unreliable.

Whiting described a remarkable increase in mutations: "It is significant that among the offspring of the relatively few treated wasps these mutations (including four visibles) have been found, while in previous work involving several hundreds of thousands of individuals not more than seven visibles have been detected."

Distraction. Copious mutations were produced with X rays. Some point mutations were produced. Moreover, this is irrelevant to charges of misconduct.

2. Beyea cites the paper of Bentley Glass (1991), which was written when Glass was about 85 y of age. I have documented that Glass's memory was fading as he failed to correctly remember the BEAR I Genetics Panel meeting activities. He claimed that differences of mutation risk judgments were resolved overnight (the evening of 6 February 1956) when all the Panelists went back to their hotel rooms and then met again the next day, settling all the major differences. The actual facts are that all the Panelists went home that day for a month. They then developed their genetic/mutation risk effects assessments and mailed these independent assessments, which were highly divergent mutation estimates (Calabrese 2017b). Thus, Glass's recollection was incorrect based on original/basic documents which unequivocally refute Glass's recollection and the Beyea story line. Beyea (2016), however, cites the Glass paper as reliable when it isn't. Again, Beyea's use of this reference makes his conclusions not only questionable but also something that cannot be relied upon.

Agreement among a subset of panelists does not preclude their changing their minds later or the fact that others weighed in later for the first time. Weaver, the drafter of the report, took all the complaints into consideration and added umbrella language under which they all could stand. That was his job as the chair of an NAS committee. Moreover, the central value of the six estimates are close, just not the ranges.

Calabrese repeats his arguments while staying blind to the criticisms that I have made about his missing key text in the report that makes his premises wrong.

3. Beyea cites a summary by Glass of the Neel research given at a 1954 AEC conference (NAS 1955). This citation has no relevance to the fact that the BEAR I Genetics Panel chose not to review the Neel and Schull final 1956 report. Neel offered this final major report to the Panel in expectation that it would be formally reviewed and even had a special mailing to Muller prior to the first Panel meeting. Muller would later claim in a subsequent letter to Neel a year later that he never read it, being too busy. The comments of Beyea are procedurally and scientifically irrelevant (see Calabrese 2020). Beyea appears unaware of this insight and the information I had access to and appears not to know the basis for selection of material. Thus, the fragmented and grossly limited perspective he offered on this matter cannot be relied upon.

Calabrese continues to ignore the language in the report that quantifies to the extent possible at the time the impact of the Neel data on the doubling dose. He does not quote Sewall Wright's correspondence with Weaver, where Wright analyzes the Neel data. Calabrese continues to ignore the fact that Neel in his autobiography states that the A-bomb data was not ready at that period of time to be determinate. Calabrese ignores the fact that Neel's reservations about estimating human risk (e.g. in the circulated chapter 15) were taken into account by Weaver's final language in the report.

Selection criteria? He should not have asked. I think Calabrese uses an opposition research protocol, selecting material that can be spun to look bad for his targets. He has stakeholder zeal and appears to have produced too many papers for careful research.

4. Beyea noted that Neel stated that the Russell (1951) mouse studies were the principal sources of genetic risk recommendations. The most important point was missed again by Beyea. This was the fact that the Russell data had a massive error in their control group

It is Calabrese who made a massive error in interpreting the Russell control group, as I argue in Part 2.

as uncovered and reported by Selby (2020), the former graduate student and long-term colleague of the Russells. Correcting the error showed that the dose response for males and females was most consistent with a threshold dose response, not a linear at low dose response (Selby & Calabrese 2023). Thus, the comments of Beyea missed the central point and certainly cannot be relied upon.

Incorrect math, as I argue in Part 2. No statistically significant threshold, even when conceptual error is retained. Removing the conceptual error shows no change in linearity at all, only an upward shift of the dose-response curve.

5. Beyea states that, "What Calabrese is essentially claiming is that linearity should be dropped because the 1956 Committee supposedly messed up the slope calculations." This is yet another misrepresentation of what I said in the 22 episodes. I did not say that LNT should be dropped in the documentary. In fact, in Episode 22, I was asked what the road forward is in light of the past 21 Episodes. What I suggested was that the EPA adopt a "model optimization" process that would incorporate key elements of the LNT, threshold, and hormesis models. I had published several papers on this topic (Calabrese 2015a, 2016) and then demonstrated how it would work during the documentary. I indicated the combined model should be adopted for use now. The three models could then compete in the scientific community based on new studies and see which emerges as the best. This is laid out in Episode 22 for the interested reader. However, the key point for this Commentary is that Beyea misled the journal and readership, once again. This time, the actions of Beyea are immediately exposed by a review of my cited papers and the watching of episode 22, and the reader can determine who is accurate: Beyea or me. Thus, how does Beyea keep making inaccurate statements that can easily be shown to be false — falsehoods that are apparently missed by the peer reviewers and still get the associate editor of *HPJ* to publish a document that grotesquely misleads the readership? At best, this is another example of confirmation bias, not only for Beyea but for the associate editor as well, and perhaps even for the reviewers. Consequently, the perspectives offered by Beyea on these matters are incorrect and therefore unreliable.

Wrong. Calabrese did say in effect that the LNT should be dropped, as I demonstrated in the introduction using the raw transcript of the video series.

This is disingenuous. Calabrese's "optimization" approach stops the linear behavior at low doses, retaining only a threshold or hormesis dose response. Slight of hand. We are given linearity above a dose threshold or hormesis response. In his optimization, radiation has no negative effects below a certain dose, i.e., a threshold.

No, as I show in the Introduction, it is Calabrese and Cardarelli who hide their call for a threshold for negative effects behind misleading language of optimization. This is an ad hoc optimization that, for instance, excludes the possibility of supralinearity. It is not based on fitting data.

Everybody is against Calabrese. From all sides.

Muller and the Nobel Prize and gene mutation: Beyea gets it wrong again

Beyea tries to claim that I was wrong to assert that Muller should not have received the Nobel Prize because he did not induce gene mutations. Beyea claims that any mutation would do. But Muller's claim was being the first to induce gene mutation as seen in the title of his key 1927 *Science* journal paper: Artificial Transmutation of the Gene (Muller 1927a). Muller would unsuccessfully try to induce gene mutation via a reverse gene mutation process in an

Calabrese ignores my point. The prize was not given for gene mutations, it was given for mutations, period. The Noble committee did not grant him gene mutations, only mutations.

effort to prove that he had not simply punched holes in his chromosomes (Lefevre 1949, 1950). Little known is that Muller & Dippel (1926) tried to clarify the gene mutation issue in the 1925-1926 time-period by assessing the occurrence of reverse mutations with over 35,000 flies even before his claim regarding gene mutations in 1927 (Muller 1927a). However, this early effort to reverse gene mutations was not successful, becoming a well-hidden finding.

The concept of chemical and radiation-induced mutation preceded Muller's Nobel Prize research by more than three decades. Prominent efforts were widely reported by numerous researchers initially led by Daniel McDougal, working in the area of plant genetics, and making major headlines just after the turn of the century (i.e., early 1900s) (Campos 2006; Kingland 1991). Others before Muller had shown that recombining broken chromosomes could lead to transgenerational phenotypic changes, as this was addressed many times by Muller. One of Muller's contemporary fruit fly competitors, James Mavor, reported that he had induced chromosome "mutation" (Mavor 1929). In his retrospective assessment, Mavor (1929) stated that "It may be properly said, however, that the induction of non-disjunction of the sex chromosome which the present writer had shown can be produced by X-rays is in effect the induction of a chromosomal mutation." Needless to say, the prior production of such chromosome mutations by Mavor did not even yield him a single nomination for the Nobel Prize. Why? Because it was not unique and not at the level of major biological significance like the alleged "gene mutations" of Muller.

Muller received immediate worldwide acclaim after the announcement of his X-ray-induced gene mutation findings, even spreading back to his campus with over 1,000 attending a lecture on his discovery at the University of Texas soon after his Berlin presentation of his findings. Muller (1928) would also strongly emphasize that all prior attempts to induce "gene" mutation had failed prior to his findings, making his discovery unique. He had to distinguish his major advances from the others, and he did this successfully. The underlying significance was that Muller was claiming that evolution was largely driven by gene mutation and not massive genetic disruptions such as "chromosomal" mutations. However, subsequent research has shown that Muller also did not induce gene or point mutations but mostly modest to massive gene deletions (Calabrese 2019a, 2019b). Thus, there is the obvious question of the legitimacy of Muller receiving the Nobel Prize for producing mutations, not gene mutations. If it were for producing chromosome-related mutations, others would have qualified long before Muller. Carlson (1989), Muller's biographer and last Ph.D. student, wrote: "[Muller] had startled his colleagues by announcing that for the first time in the history of life on

Distraction. The justification for a Nobel Prize has nothing to do with misconduct. Moreover, work by others before credit is assigned is common. (See Stigler's law.) As Calabrese knows, what Muller did was to develop an experimental method that allowed copious mutations to be produced and measured. Previously, it was very difficult to quantitatively assess radiation induced mutations. Often in scientific work, a new experimental technique is revolutionary and is rewarded with attention. Calabrese stands alone in this regard that Muller did not deserve a Nobel Prize. It is a petty attempt to frame a reader's mind to think that Muller's LNT work must be flawed. Moreover, Calabrese is out of date. Radiation is known today from genomic analysis to produce multisite mutations and insertions as well as deletions (indels), which play a role in evolution (Redelings et al. 2024), "especially effective when organisms are faced with abrupt environmental challenges." (Albalat and Cañestro 2016): See also: (Satoh et al. 2020) (Matsuda and Tanabe 2025). As with Muller's ratchet, Muller's concepts were ahead of his time.

See above. Muller's Nobel Prize citation does not claim he produced gene mutations. Calabrese cannot face this. Also, Muller did produce some de novo mutations.

One can never accept what Calabrese claims one of his targets says without checking.

If Muller was claiming that evolution was largely driven by gene mutation, then he would be in good company today. Calabrese may have written this differently than he intended

Mostly deletions, but also insertions and clustered mutations (Adewoye et al. 2015), an advantage for evolution in unusual times. (Albalat and Cañestro 2016) This paper, "Evolution and gene loss" has been cited 800+ times, but not by Calabrese I checked.

Muller's technique produced copious mutations making his discovery unique, as noted by Plough and Crow.

earth, the hereditary material of living organisms — in this case, the Genes of fruit flies — had been artificially mutated." There were many other similar praising declarations by contemporary genetics researchers placing Muller's gene mutation findings in a unique place in the history of science. Beyea boxes himself in and there is no escape. The Beyea position on this topic is unreliable. It was this type of thinking and unwarranted argument, so evident in his first paper, that made it simply unproductive for me to respond to this second paper regarding me published many years ago.

It was unproductive to respond to my 2017 paper? Or, was it that he had no adequate response?

Beyea ignored serious ethical failures of the NAS and the BEAR I Genetics Panel

Beyea failed to disclose that the 1956 NAS BEAR I Genetics Panel "Report to the Public" was not written, approved, or even read by the Panel prior to its release to the media/public. Yet an entire article was written on this matter and published in *HPJ* (Calabrese & Giordano 2022a). The Report to the Public was written by an independent third party. It was represented by the NAS as being the Report of the BEAR I Genetics Panel. Many letter exchanges among the panelists document this fact. These letters also revealed major concerns, frustration, and anger with such actions, including there being important errors in that Report to the Public, which was falsely attributed to them (Calabrese & Giordano 2022a). Yet, none of the Panel members came forward to correct the record. The misrepresentation of this Report to the Public represented a major ethical failure of the NAS President, Detlev Bronk. Likewise, the failure of the Panel to correct the record, rather than to keep it hidden, is also a major ethical failure. While Beyea fails to use primary sources, the Calabrese and Giordano (2022a) paper provided the detailed documentation of these ethical transgressions. The failure to report such ethical failings of the President of the NAS and the Panel as reported by me shows that Beyea's representation of ethical issues is not reliable.

Calabrese has shifted to a digest of the genetics report. It is standard practice at NAS to produce summarizing reports for the press and public. Only the committee chairs review the professionally written summaries for the press and public. In this case, the genetics report was only one of six discussed. With over 100 individual committee members, it is clear that review by each of them is impossible. The article by Calabrese & Giordano makes a mountain out of a molehill about 1 error made in a statement repeated 8 times, a frequency that they never admit. Mistakes happen. This one was found by members of the 1960 committees long after the 1956 report had been circulated. The 1 in 8 errors was not repeated again. Calabrese should think of the errors that he has made and never corrected, even in subsequent publications.

Beyea missed the obvious: Calabrese is not just a Muller critic but reports positive activities

Beyea often misses important papers and misleads himself, reviewers, editors, and now readers of the *HPJ*. For example, Beyea failed to note that I published an article about how Muller came to the defense of his chief scientific rival and critic, Professor Lewis Stadler, University of Missouri, for issues concerning an FBI investigation into communist party activities in the late 1940s during the McCarthey Era (Calabrese 2017a). Muller showed amazing support and leadership in trying to help Stadler save his career. In the end, Stadler's career was saved, thanks in part to Muller. Beyea failed to show to the reader that I not only discovered this episode in the lives

One article compared to dozens of personal attacks on Muller doesn't balance out. It does not excuse his smears. I also praised Calabrese for certain strengths, but I am sure he doesn't think that makes up for my critiques.

of Muller and Stadler but also obtained primary source material that documented it and wrote a peer reviewed article on the topic (Calabrese 2017a). If I were only looking to find fault with Muller, this article never would have been written and published. In fact, I was the investigator who discovered it and helped to make it part of the public record, something that Schwartz (2008) and Carlson (1981) did not do. Again, Beyea failed to share this information with the readership.

Calabrese defines peer review very broadly as including screening by an editorial board: "the reading of manuscripts and decision on them will be divided among members of the editorial board, of whom there is a sufficient number to so divide the work …

This is not what peer review means to readers today.

Muller ignores peer review to promote his Nobel Prize chances: Beyea missed the key references again

Beyea agrees with my assertations that Muller did not subject his Nobel Prize research papers to peer review (Calabrese, 2025; Calabrese & Giordano 2022b). Beyea then tries to excuse the actions of Muller, saying that peer review was not widespread and established. In fact, he makes the claim that it took until the 1970s for this to occur, citing one recent reference. A detailed review of journal administrative primary sources and journal articles indicates that many of the major journals had adopted a peer-review process by the 1920-1930 time-period. Well established documentation exists for journals such as *Plant Physiology* [start of peer review (PR)-1925] (American Society of Plant Biologists website 2025), *Journal of Bacteriology* (start of PR-1917) (Porter 1974), *Journal of Experimental Zoology* (start of PR-1904) (Harrison 1945), *Journal of General Physiology* (start of PR-1918) (Andersen 2005), *Journal of Physiology* (start of PR-1927) (Bynum 1976), *Journal of Experimental Medicine* (start of PR-1905) (Flexner & Flexner 1993), *American Journal of Botany* (start of PR-1933) (Smocovitis 2014), *Genetics* (start of PR-1917) (Dunn 1965), *Immunology* (start of PR-1916) (Emrich 2016), *The Biochemical Journal* (start of PR-1912) (Biochemical Society 1985), *Journal of Clinical Investigations* (start of PR-1924) (Brainard 1959), and many others. Some of the journals provide detailed information concerning the number of manuscripts received, accepted and rejected. (e.g., *Journal of Experimental Medicine, Journal of Bacteriology* and others). Also, although not specifically cited due to space, documentation was obtained by purchasing numerous letters (e.g., George Shull, first editor of *Genetics*; Simon Flexner, editor of *Journal of Experimental Medicine*; and James McKeen Cattell, editor of *American Naturalist, Scientific Monthly, Science*), peer-review files, and other relevant documentation for many of these journals, with several journals being extremely copious. For example, these letter exchanges provide the actual peer review and basis for acceptance, revision, or rejection of manuscripts. In the case of the *Journal of Experimental Medicine*, the materials extend for multiple decades (1915-1941) with detailed assessments of hundreds of papers that I possess.

This is a silly digression. Calabrese blames Muller for publishing in Science, when everyone else did. No one else cared that external peer review was not mandated at Science. Calabrese disagrees with yet another historian, this time Melinda Baldwin, who wrote, "…it was only in the late twentieth century that peer review came to be seen as a process central to scientific practice." (Baldwin 2018).

Thomas Hunt Morgan, the head of the Fly Room at Columbia where so many great geneticists arose, including Muller, published two dozen times in Science prior to 1927, Muller had published there before and so did CC Little (11 times by 1927), AH Sturtevant (9), M. Demerec (1), Sewall Wright (1), all of whom were on the 1956 NAS genetics committee with Muller. It was highly respectable to publish in Science, even in the period before 1940. Articles reached a wider audience than specialist genetics journals.

I have come across no one other than Calabrese who thought that Muller did anything wrong by publishing in Science magazine. The fact that Science was a high-profile journal and an article in it would reach a broad audience, not restricted to geneticists, made it an attractive choice for important research of interest to the wider world. This diversion here by Calabrese is another example of him overreaching to make Muller, his obsession, look bad.

Morgan, Muller's advisor, published more than 20 papers in Science over many decades as I found in a Google Scholar search. Calabrese is on a diversionary trip here.

Science was a commercial operation at the time. The publisher would get reviews as he felt they were needed. Calabrese's identification of but one example from the entire files of Science's publisher is consistent with that mode of review.

Nonsense. Publication in Science by geneticists was a key part of their culture. Calabrese neglected to check who published where. Calabrese makes the error of assuming that today's practices are the same as those in 1927. Peer review in that period did not match peer review today and was not universal. Moreover, peer review was not considered fundamental, as shown by so many geneticists publishing in Science.

"... Cattell and his board solicited many of the papers that today would be published as articles or reports." (Sokal 1980).

No. Muller published his most important paper in the journal that would give his groundbreaking work wide attention.

Muller's advisor, Thomas Hunt Morgan, was on many of these above editorial boards (e.g., *American Naturalist, Genetics, Journal Experimental Zoology*) and helped to establish and participate in the peer review process while Muller was working under his direction. In addition, Albert F. Blakeslee, who reported radium-induced gene mutation in *PNAS* some 6 mo prior to Muller's paper in *Science* (Gager & Blakeslee 1927), was used by *Science* journal as a peer reviewer for genetic research performed in the laboratory of Thomas Hunt Morgan on 6 December 1926 (Blakeslee 1926) 7 mo before the key Muller *Science* publication. This finding alone is sufficient to discredit the assertions of Beyea. Likewise, Blakeslee was also a reviewer for *Genetics* as seen in correspondence with the editor-in-chief (Blakeslee 21 & 24 September 1928). Thus, it is important to note that the single reference cited by Beyea on peer review was of a general nature and failed to address the occurrence of peer review in relevant biological journals in the Muller era, especially in the area of genetics. Thus, the information provided by Beyea was grossly inadequate to address the key questions, whereas what I have provided is far more properly designed to answer the question, by getting the actual peer reviews from the preserved files of the editor at the very start of the journal *Genetics,* a finding that discredits the Beyea statements on the matter. Beyea provides an inaccurate description of the scientific culture that Muller was educated and trained in and how it had peer review integrated within it. The information that Beyea provided therefore is unreliable for the question raised, and this should have been challenged in the peer review process.

Muller was thus acculturated into the peer review process as a graduate student, starting in 1911. In addition, the key journal, *Genetics*, started peer review just at the time Muller had finished his Ph.D. The comments of Beyea are inconsistent with the historical record. The fact remains that Muller was well aware of the peer review process, had participated in it, and avoided it for his most important paper.

Of telling importance to this issue is that the owner and editor-in-chief of the *American Naturalist* and *Science* journals, James McKeen Cattel, asked Muller to be a guest editor for the *American Naturalist* journal and to acquire excellent papers that had been presented at the Fifth International Genetics Congress in Berlin, September 1927. This was the meeting where Muller presented his Nobel Prize research for the first time. Muller was a guest editor who was to oversee the review process and make decisions on acceptance or rejection of manuscripts [Muller letter to Demerec, 26 October 1927 (Muller 1927b)]. Yet, Muller curiously neglected to have his own Nobel Prize winning research paper peer-reviewed for subsequent publication in the *American*

Naturalist journal. Muller was also on the editorial board for *Advances in Genetics* where Demerec was the editor-in-chief and was also involved with the manuscript review process (Demerec letter to Muller, 11 May 1953). The collective findings show that the argument of Beyea that peer review was a recent phenomenon fails to match the evidence provided here. As in many of the other examples in this paper, such information is best obtained from primary sources, which is information Beyea did not use.

The external peer review process we know today, with experts generally separate from editorial boards, reviewing papers with their comments shared with authors, took a long time to become established.

Calabrese picks a subset of examples, without a denominator. Bad math. Do not accept any Calabrese statements about historical documents without checking. That's a lesson I have learned from reviewing his work. And do not accept his supposed contradictions of historians such as Baldwin, Beatty and Jolly.

Why not publish in history of science journals? The answer is easy to understand

Beyea argues that I should have published in history of science journals. The problem with Beyea's argument is that citations of papers in even the most prominent history of science journals and with the most prominent historians are grossly anemic. A check of the H-index was made for five of the most prominent US historians of science and the addition of Beatty from Canada who Beyea relies on, and who have published in the area of the history of cancer risk assessment. Their scores ranged from 5-15, whereas my H-index is 112. While these papers can be excellent, they tend to have limited circulation, with little impact outside their narrow disciplinary domain. If I were to follow the Beyea line of argument/suggestion, then my well-cited papers would be effectively hidden under an academic basket, read and cited by few, thereby having far less impact. Thus, Beyea's line of argument is not convincing. I have published my papers in a diverse but relevant range of high-level journals that reach most scientists, with all being cited in Pub Med and the Web of Science and other indices.

If Calabrese as an amateur is to challenge professional historians, he should publish those challenges in professional history journals so his claims can be seen and judged by historians. Without such review, no one should accept his criticisms at face value.

Scientists are not generally science historians. What do science historians think of Calabrese's work? I have not heard a single positive word. Has Calabrese? He criticizes Muller for avoiding relevant review, but thinks it OK for him to steer away from what might be critical reviews by science history journals. Actually, he should tell us if he has ever submitted to a history journal.

More on the Stern-Uphoff case: The critical experimental data have been missing for nearly 80 y

These authors promised to publish a manuscript that would be "a more detailed account of the work" in addition to being part of a single table in a paper lacking methods and materials, experimental protocols, etc., all usually considered elements of a scientific paper. This paper became the key foundation for both the BEAR I Genetics Panel switch to linearity and the work of Edward Lewis to support linearity. Yet, the findings were never published and have been missing nearly 80 y. They were relied upon when making far-reaching decisions---yet Beyea failed to mention this, providing another reason why his conclusions are unreliable.

This is nonsense. The methods had been presented in previous papers and reports by the group, which were cited. Standard practice. Uphoff replicated a previous experiment, which the previous experimenter, Caspari, helped to explain in a three-day visit to the lab. Upon receiving a draft of the short paper that Calabrese trashes, Caspari wrote that it should be published as soon as possible and that it settled the question of radiation producing mutations over a protracted period. This correspondence from the files of the American Philosophical Society is never mentioned by Calabrese.

Challenges along the way: Muller's daughter refuses to let Calabrese have access to key documents in the Muller files at the University of Indiana

Hidden among the many critics are strategies to prevent legitimate research. For example, I have made

Had I been asked, I would have urged Muller's daughter not to let Calabrese obtain research materials about her father, because he misuses them with regard to

Muller in my view, with whom he appears to be obsessed, at least until such time as he is working with a professional historian bound by standards such as expressed by the AHA.. Library research materials should not be freely given out to someone with a history of doing opposition research to support a stakeholder claim. Would Calabrese be willing to hand over all his correspondence, drafts and research material to Muller's daughter or to me?

Hmm. No claim made by Calabrese about financial misconduct? In episode 15 after about 16 minutes, Calabrese says, "It's all about money, money in these these these radiation geneticists." The meaning of my section heading about financial misconduct was defined by the examples given in the subsequent text. Researchers, as Calabrese knows, have to obtain and manage grants and funding. These are financial practices of science that are a major responsibility of principal investigators and science managers. To imply that Committee members changed their views on the science, as Calabrese does in his papers and in the next section, to obtain funding from Weaver is to charge financial misconduct in my view. As I wrote, "Charges of financial conflicts can be made about most researchers because they need funding to produce results."

If the Committee accepted a bribe, I call that financial misconduct. If someone offered a bribe, I call that financial misconduct. No proper definition?

No opportunity for self-defense? Calabrese always has a defense for anything and everything he does.
"

Calabrese is trying here to imply that the Committee members changed their minds after Weaver's offer to go to bat with The Rockefeller Foundation. Note that this occurred at the second meeting. At the first meeting, the LNT philosophy was presented in the meeting agenda and discussed without issue early in the first meeting. No dissent ever about that. Dissent was about the strength of the linearity,

repeated requests to obtain/purchase documents from the Muller collection within the University of Indiana Library. However, on multiple occasions the library has indicated that Muller's daughter refused to permit me to obtain certain research materials from this state-supported University library that were donated about 60 y ago when the daughter was a young adult. Yet, the information that I have requested was shared with Carlson. On this matter, the FBI files on Muller that I have finally obtained have opened up entirely new areas of inquiry that have led to new manuscripts (Calabrese et al. 2025).

More false charges

In 2.6, Beyea asserts that a false claim was made that the 1956 NAS committee "engaged" in financial misconduct. Beyea never states who, when, where, and what was the "claim." Beyea never defines the term "financial misconduct," nor does he define the term "engaged." The Beyea Commentary that followed the allegation provided no evidence that was even remotely relevant to the allegation. Yet the reviewers and editors failed to require that Beyea offer a clarified and properly defined set of allegations with supportive evidence to which an accused person could respond. In the present case, how are the accused supposed to defend themselves? The Beyea approach is simply to accuse and then offer nothing that is specific that could be refuted. When a "crime" is committed, it is important to know when, where, and how it occurred so that an accused person has an opportunity for a legitimate defense. In the case of Beyea, he failed to properly define the accusation, allowing no opportunity for self-defense. This situation will ensure that his accusation lives on.

When is a bribe a bribe? The case of Warren Weaver — Why Beyea's explanation cannot be trusted and is not reliable

Beyea acknowledged that Warren Weaver, Chair of the BEAR 1 Genetics Panel, made a "bribe offer" to the Panel based on the book of Berg and Singer (Berg & Singer 2003). The bribe statement was made very early in the morning of 5 February 1956 (page 35 in the transcripts). The statement was made within the context of Weaver being the Director of Research at the Rockefeller Foundation (RF) for over two decades and having been in charge of awarding research and programmatic grants to various members of the BEAR 1 Genetics Panel. He also understood how important grant funding was to academic researchers. It is their professional lifeline. Weaver was now in charge of the BEAR I Genetics Panel, which was also being fully funded by his organization, the RF, that gave the money to the US NAS. It is also important to appreciate that the President of the NAS, Detlev Bronk,

was a long-time member of the RF Board of Directors as well as the President of the Rockefeller Institute for Medical Research (soon to have its name changed to the Rockefeller University). This is correct; Bronk held both Presidencies at the same time and would find himself in a highly conflicted position. Beyea attempts to dismiss the provocative bribe-like statement of Weaver. Beyea claimed that Weaver didn't really mean what he said. According to Beyea, what Weaver was really trying to do was to show his "determination to get a report finished." The support for this statement, according to Beyea, was from the book of Berg and Singer (2003), a biography of George Beadle, a BEAR I Genetics Panel member. With that as backdrop, let us now read the paragraph from Berg and Singer (2003) (page 230) that contains the bribe reference:

"The panel met again in February 1956 in Chicago. Weaver understood that inspite of the disagreements within the committee, they had to construct a report that all panel members could approve. He came to Chicago prepared to succeed. He proposed that the initial reports of all the panels, including genetics, be brief and incorporated into a single, plain-spoken document; a draft of the genetics section was in his briefcase. He also held out a bribe; the Rockefeller Foundation would provide substantial support for fundamental genetic research so that in future, more knowledgeable recommendations could be made. Weaver read from the draft at length, with minor interruptions from the geneticists and a few on his part to apologize for any errors in science. Beadle, who was pretty quiet during most of the presentation, finally gave him the go ahead: 'You just keep going. We don't need any professional popularizers, all we need is you. We will put the commas in and make the corrections and put the qualifications in and we will have a fine job here'. Muller agreed. Then Weaver knew that he would have a finished brief report within months."
Let us now consider what Muller said. Immediately after the comments of Beadle, Muller stated (page 34 in the transcripts): **"I think we would all much rather have you do it than any professional popularizer, excepting the word 'professional' as applied to popularizer."**

What was occurring was that Weaver was expecting each of the six NAS panels dealing with nuclear issues to produce a brief and easily understood document. These reports would be assembled into one NAS report in a well-coordinated and timely fashion. Taking the initiative, Weaver created what might be a first draft of such a popular summary report, depending on the response of the Genetics Panel. In his opening remarks, Weaver emphasized that their Panel report

What is Calabrese making up here? The Rockefeller Foundation had no agenda with dose response. They supported a study on bomb testing fallout, which was a public concern at the time.

This was the opinion of scientists Berg and Singer, which I quoted: "...the author/scientists Maxine Singer and Paul Berg attributed Weaver's "bribe" offer in the second meeting as reflecting Weaver's determination to get a report finished..,. "

Calabrese claims that I misquoted Berg and Singer later in this section, but he repeats the very same message here: "He came to Chicago prepared to succeed." ,,,,"He also held out a bribe; the Rockefeller Foundation would provide substantial support for fundamental genetic research so that in future, more knowledgeable recommendations could be made.;"

In episode 15 after about 16 minutes, Calabrese says, "It's all about money, money in these these these radiation geneticists." He further claims falsely that Weaver said, "he tells them, and it's right in the transcripts, he said, If this report comes out the way we hope it does, there'll be lots of money, free money, flexible money for geneticists. The transcript says, "...I will bat my head in the Rockefeller Foundation to try to get a very substantial amount of free support for genetics if at the end of this thing we have a real case for it."

Weaver was not talking about the way he or Rockefeller wanted. The section in the report that talked about funding based the case on correcting a funding imbalance compared to other research. Nothing was said about dose response.

How does Calabrese know what Weaver, a mathematician, hoped? Shredding a medical community paradigm? This committee was about genetic mutations, with no mention of cancer or medicine. Calabrese acts like a novelist here, not a scientist. This is a typical example of his putting words in people's minds, asking us to believe that he can read the minds of dead people.

Weaver and the other committee members are trying to deal with genetic effects of radiation, including from fallout. I have not seen and Calabrese has not shown any member of the Committee talking or writing about changing risk assessment. For them it had long changed. They did not accept a threshold for genetic effects. They wanted the public to know their concerns about genetic effects of radiation, and they set a dose standard. No recommendation about risk assessment. Nothing in the report about cancer or other non-genetic health effects.

should claim that the dose response was linear down to a single ionization, there was no repair, and the damage was cumulative and irreversible. In fact, it sounded as if Muller had written the script for Weaver. Weaver used his opening remarks for what he hoped to be the Panel report to shred the long-time dose response paradigm of the medical community, which was the threshold dose response model. The statement of Weaver therefore was far from trivial. It was the opening salvo in the battle over the future of hereditary and cancer risk assessment. What Weaver heard was very encouraging. The immediate comments of Beadle and certainly of Muller indicated that if Weaver wanted to write the report, that was fine with them. Weaver was in full alignment with what Muller had been advocating since 1930. However, now Muller would have the power of the RF and the NAS behind him. No one complained. The implications were that it was better for Weaver to do this than hiring an independent third party to write this popular document. Based on what Beadle and Muller stated, the role of the Panel was likely to be advisory, ensuring that Weaver's brief summary was accurate, well written, and fully supportive of their views.

About a minute later, Weaver offered the bribe statement on page 35 of the transcript:

"There may be some very practical results — and here is the dangerous remark — don't misunderstand me. We are just all conspirators here together. I am not talking as an officer of the Rockefeller Foundation but I will bat my head in the Rockefeller Foundation to try to get a very substantial amount of free support for genetics if at the end of this thing we have a real case for it. I am not talking about a few thousand dollars, gentlemen. I am talking about a substantial amount of flexible and free support of genetics. I will bat my head off to get it at the end of this if we have a really good case for it."

Several pages later Weaver stated that his current draft is now eight pages, and he would expect that the final draft would be less than 15 pages. He then stated that "Now this is what I am supposing, but at the moment I am asking you what are the last two or three pages are going to look like." Dr. Douglas Whitaker of the National Research Council and the Rockefeller Institute of Medical Research then noted that the Pathology Panel was thinking along the same lines, producing a short report, hoping to get it done in about a month.

Within this context, it seems clear that (1) Weaver wanted to strongly advocate for a radical change in radiation risk assessment, a switch from a threshold to a linear model; (2) the report should be short but readily understandable, and non-technical; (3) that Weaver wanted to quickly move the process forward; and that (4) the Panel

was highly supportive and could be useful in making sure that Weaver captured what was needed.

Why would Weaver make such outlandish and insulting bribe-like comments to the Panel may be hard to understand. Why would he say that "…here is the dangerous remark….We are just all conspirators here together." Then Weaver talks about new funding for geneticists that is significant, flexible, and substantial. In the end, the statement of Weaver to a Panel that he is directing is highly inappropriate. Panel members, in fact, should have first immediately challenged Weaver on the dangerous remark and conspirator comments. What exactly was he talking about, as if they didn't know? Why were these comments linked to funding? Not a single panelist challenged him or requested clarification. The panel did no favors for Weaver or themselves by remaining quiet. Further, no one reported these bizarre bribe-like comments of Weaver to President Bronk. The comments are scandalous and could have been devastating had they been shared with the media. The fact that no member of the panel registered a challenge or complaint to Weaver or Brock is very telling.

Weaver knew that the Genetics Panel report would be the so-called lynchpin of the entire RF-NAS effort with respect to the general public. He especially noted this strong belief to the Panel in his opening remarks. Thus, success or failure would depend on how transformative their report would be. Given Weaver's position in the RF and its funding of the entire endeavor, and under the banner of the US NAS, there was heightened opportunity for the Genetics Panel to be the transformative entity, especially with some of the star power on the panel. However, Weaver also wanted to ensure success by having a Panel that was all on the same page with no minority report muddying up the process. Within this context, Weaver's comment makes sense. He was sweetening the pot, holding out the promise for a vast increase in research funding for the genetics community, including the big players on his Panel that were already receiving such funding as well as keeping possible dissenters in line.

The interpretation of Beyea has no support. In fact, this is not what was written in the Berg and Singer (2003) chapter as shown here. Furthermore, his write up fails to place the comments of Weaver and Panel members in context. Beyea should have used the primary source material rather than relying on the book chapter of Berg and Singer (2003) that itself also failed to provide the broader context. Surely, the editors of the *HPJ* cannot realistically believe that the information provided by Beyea on this matter for the reader was adequate to make an informed judgment. He presents a view that cannot be relied upon.

Based on their language, I think Berg and Singer see things differently than Calabrese, as do I. For a bribe to be a real bribe and not an exhortation, the briber must be asking the respondent to do something they ethically should not do, asking them to avoid their duty. Here, Weaver in our view is trying to get the Committee to actually do their duty and produce a report, to stop squabbling and settle down. This is a universal motivation among NAS chairpersons, based on my experience. But readers will have to decide for themselves.

Berg and Singer, participants in NAS studies themselves, did not see Weaver's actions as scandalous.

Producing a convincing report and not fighting to a standstill would not seem to me to qualify as a bribe target. In fact, that was their duty in the first place. It was more like holding out a reward for doing one's duty. Moreover, Weaver himself was not the beneficiary. It was the public who would be the beneficiary of a convincing report. Nevertheless, Weaver's conspirator's remark was politically stupid to make for a former president of the AAAS, albeit made likely I presume to amuse. The remark could have been used negatively by the AEC. The meeting was recorded, and committee member, Shields Warren, also chair of the NAS pathology Committee, who was close to the AEC having been the head of their Division of Biology and Medicine, could have asked to review the tape, because he had been in Japan and could not attend.

Note that as I wrote in 2017, the major funding for genetics came from the AEC. The Carnegie foundation was also a major funder, along with Rockefeller..

I note that I introduced the bribe terminology from Berg and Singer in the interest of not withholding information that might be used to counter my interpretation. I did not want to be hypocritical in pointing out examples where Calabrese showed selective stakeholder perception.

Editor Brant Ulsh has dealt with this, rejecting Calabrese's objections (Ulsh 2026). I had asked colleagues to review my drafts, and I identified them in my article, which I was not required to do. Do not take Calabrese's version of his relationship with George R. Hoffman, a coauthor of mine, as truth. It is his side of a story. We know from the first part of Calabrese's rebuttal that he feels everyone is against him and he takes no responsibility for any of it.

George R. Hoffmann has always spoken respectfully to me of Dr. Calabrese, although expressing disagreement with him professionally on hormesis findings in animals, as he has written in the literature (Zeiger and Hoffmann 2012), concluding that the Ames test uses by Calabrese is inappropriate for demonstrating hormesis in bacterial mutagenicity experiments. In any case, my 2025 paper is about radiation and has nothing to do with anything like the Ames test.

Hoffmann has also expressed disagreement as to some of Calabrese's claims about the character of people known personally to Hoffmann, such as William L. Russell, or through knowledge of historical papers that Hoffmann solicited as editor of Reflections in Mutation Research, a component of Mutation Research Reviews (Hoffmann 2019). Because I am not a geneticist, I especially wanted my manuscript reviewed by a geneticist, especially one like Hoffmann who has experimentally studied mutation following ionizing radiation. My paper was about science history and charges of misconduct, as well as my pointing out that the modern views of LNT are determined by modern experimental data. And, as I said, Hoffmann always spoke respectfully of Calabrese to me, which is one reason I think Calabrese has a role to play in these debates..

I wonder if Calabrese has even disclosed the names and affiliation of his personal reviewers, let alone their past rivalries. I find this all to be a way to look injured and generate sympathy. Disputes over methods in science between former colleagues, such as between Hoffmann and Calabrese, play out in the literature all the time. My article had nothing do with any of Calabrese and Hoffmann's joint papers and work product.

Beyea's failure to disclose consultant association

Beyea noted in his acknowledgment section that George Hoffmann served as a consultant for his paper published in *HPJ*. What Beyea failed to tell the reader was that Hoffmann had a prolonged professional relationship with me in the area of dose response and hormesis. The information that Beyea should have disclosed in the paper is as follows. Hoffmann served as a consultant for about 6 y in conjunction with a long-term grant on hormesis to the University of Massachusetts with me as the Principal Investigator. The consulting activities developed following an initial invitation by Hoffmann, editor at *Mutation Research*, for me to write a peer-reviewed article on hormesis in late 200l. The paper was written and passed peer review and was then published in 2002 (Calabrese 2002). Two years later Hoffmann invited me to give a seminar (20 October 2004) on hormesis and meet with students at Holy Cross College, Worcester, MA, where he was a long-time professor in the Biology Department, teaching genetics. After the seminar and meeting with students, Hoffmann hosted a reception in my honor at his home, an activity well attended by faculty and students. These positive interactions and mutual professional interests would soon lead to me to invite Hoffmann to become collaboratively involved on my Air Force-funded research grant as a consultant. Hoffmann became very involved with the project, in ways far exceeding normal consultant activities, as he became a coauthor on six high level papers (Calabrese et al. 2006, 2007a, 2007b, 2008, 2010; Nascarella et al. 2009), participated in multiple ways in my UMass hormesis conferences, wrote commentaries and articles on hormesis (Hoffmann 2009; Hoffmann & Stempsey 2008), and even made a trip to Mexico to give a presentation on hormesis as a spin-off activity of this association. I also invited Hoffman to be the moderator of sessions at the annual Hormesis Conference which occurred in 2007, 2009, and 2011 (see International Dose Response Society website, Conferences section). Of further significance is that I invited Hoffmann to give one of only two plenary addresses in 2008. In addition, Hoffmann offered considerable assistance to one of my Ph.D. students (i.e., Marc Nascarella) on an hormesis dissertation. Nascarella formally acknowledged the assistance of Hoffmann in the Acknowledgements section of the dissertation. Thus, Hoffmann and I had developed an unusually friendly and productive professional relationship at a high level of academic involvement. Hoffmann regularly attended and fully participated in the hormesis project's research team meetings, being well known to all team members. All research ideas, activities, manuscripts, challenges, and group strengths and limitations, funding strategies, and related matters were shared with him. Hoffmann also became very actively involved in

the activities of the International Dose Response Society under my direction.

He was fully aware of the criticisms of my research and me as well as strategies/plans on how to respond to such criticisms. For example, Hoffmann was fully aware of the criticisms of Elliott (2006) dealing with his proposed environmental ethic that targeted me, as noted earlier, and my rebuttal commentary (Calabrese 2007). Hoffmann was fully informed by me when University of Massachusetts administrators placed me on "trial" in an academic misconduct investigation in the spring of 2011 as a result of the administration being sent an anonymous letter along with a recent article by Professor Kristen Shrader-Frechette (2010) that was highly critical of me both with respect to ethics and scientific issues, as discussed earlier. In my defense, I personally prepared/wrote a 50-page document that I asked Hoffmann to confidentially review and offer comments on how it could be strengthened. Hoffmann also knew that I had been vindicated during the UMass "trial" based upon the detailed information that rebutted the Shrader-Frechette article, with him knowing the entire set of information provided about her work. Yet, Beyea cited this episode to draw readers to the compromised Shrader-Frechette paper. In addition, the Shrader-Frechette paper had no relevance to the LNT documentary, the focus of the Beyea paper, and the LNT issue. My relationship with Hoffmann was not only one built on mutual scientific interests but also on trust, with all such details on the most sensitive issues being shared with him.

One of the hormesis projects that was undertaken during Hoffmann's role as a consultant at UMass involved assessing the occurrence of hormesis for various mutational endpoints, with one focused on the manifestations of hormesis in the Ames assay. A manuscript was developed on this topic and was accepted at *Mutation Research* (Calabrese et al. 2012). This occurred during a time when Hoffmann was fully involved in team meetings/research activities. Without my knowledge and that of anyone on the UMass team, Hoffmann and a colleague published a rebuttal to our hormesis and Ames assay paper (Zieger & Hoffmann 2012). This was an unheard-of act and especially shocking given the nature of our professional and personal relationship and that he did this while being a long-term consultant, professional friend, and confidant, advising me and my research team on these matters. During the development of his rebuttal paper, Hoffmann participated in group meetings, was exposed to our research plans and publication activities, including our mutation paper, all the while charting his rebuttal. It should be noted that, in the Zieger & Hoffmann (2012) publication, Hoffmann inexplicably neglected to report any "Conflicts of Interest," thus raising the issue of a possible

This hormesis work and collaboration has nothing to do with the content of my 2025 article, which is about claims of scientific misconduct..

Calabrese does not start claiming scientific misconduct until about 2011.

She was not critical of Calabrese about his historical research.

Completely misleading. Why did I list a paper by Shrader-Frechette? Not to indicate it was true or false, but to indicate that he could respond to charges of misconduct. I wrote: "Fortunately for Calabrese, he can respond to charges about character (Shrader-Frechette 2012), unlike the dead scientists whose reputations have borne the brunt of his unfounded criticisms."

Calabrese does not understand how his attacks on early geneticists has alienated him from geneticists and others. These attacks are what is unheard of.

The "without knowledge" part is Calabrese's side of the story, Not Hoffmann's.

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Calabrese is a great storyteller. Treat his ethical critiques of those who disagree with him as potentially biased

Distraction. The HPS documentary was about radiation and the LNT, not the work that Hoffmann did in collaboration with Calabrese.

Hoffmann had no insider knowledge about Calabrese's denigration of historical figures and his tries at science history. Hoffmann had knowledge of the field of radiation -induced mutations, as well as knowledge of historical figures.

We have here another example of a ham sandwich being indicted. Calabrese got to spin the story to these unnamed ethicists. Let me know who they are and I will fill in the missing information for them.

This is another example of Calabrese accusing his critics of ethical failings, one way or another. Suppose I had not disclosed my personal reviewers. What recourse could he have had then? The irony here is that Hoffmann was always tempering my language about Calabrese in his suggested edits.

The editor considered the COPE Retraction Guidelines in this case and did not accept Calabrese's call for retraction. But this was a clever way to try to shift attention from his overlooking of key historical text and the developing of false premises on which allegations of misconduct were based.

Calabrese pushes the concept of conflict of interest to new heights: influences on the author by colleagues reviewing their papers, regardless of relevance and timing. Why not demand disclosures of an author's interactions with colleagues over an entire lifetime, all that may have affected them? For instance, I have heard many negative comments about Calabrese's character over the years from colleagues, but not from Hoffmann. Should I have disclosed them?

ethical failure for non-disclosure. However, Hoffmann acknowledged his association with me in his other cited papers (Hoffmann 2009; Hoffmann & Stempsey 2008). As a result of these circumstances, it was necessary for me to terminate Hoffmann from my project and related activities. Hoffmann therefore had been a UMass hormesis project insider, with unique knowledge of me and all aspects of the hormesis project and my other professional activities. When Beyea used the services of Hoffmann as a consultant to comment/advise on his article about my interview for the HPS documentary and my long research involvement on the historical foundations of cancer risk assessment, he was surely obligated to inform the journal editors, reviewers, and the readership of the nature of Hoffmann's relationship with me and how and why that relationship ended. Hoffmann clearly has long-standing historical and personal "skin" in the game and could have brought strong negative bias and unique "insider" knowledge that affected Beyea and influenced his personal and professional criticisms of me. Yet the *HPJ* readership was deceived by Beyea, creating a false impression of Hoffmann's background. The outline of this story, without names and institutions given, were shared with multiple ethicists, with each one independently concluding that Beyea needed to disclose relevant information about the Hoffmann incident in his acknowledgement section, something he did not do. Failure to properly disclose this information represents a serious ethical failing of Beyea. It creates a situation in which any possible damage to my personal and professional reputation cannot be adequately corrected, with — for example — a subsequent erratum/correction.

The issue of Beyea's failure to disclose the Hoffmann relationship with me is relevant to the COPE Retraction Guidelines which state: *"Retractions might be necessary for unethical research practices, compromised peer review, or undisclosed conflicts of interest that could bias interpretation of the work or recommendations by peer reviewers"* (italics added).

The above detailed discussion of the Hoffmann-Calabrese interactions is directly relevant to the statement by COPE concerning, "undisclosed conflicts of interest that could bias interpretation of the work or recommendations by peer reviewers." It shows that Beyea failed to disclose conflicts between Hoffmann and me about my research and character attributes that were also targeted by Beyea and that these could have biased the interpretation of my research and recommendations by peer reviewers. Also not acknowledged by Beyea is that Hoffmann had longstanding personal/professional relationships with a number of the key geneticists whom I severely criticized and determined to have committed scientific misconduct in my articles and in the documentary. These relationships

of Hoffmann with the key leaders started when he was a graduate student at Oak Ridge National Laboratory. In fact, Hoffmann would share information in group UMass conversations concerning the camaraderie as a graduate student that he experienced based on numerous Sunday morning hikes led by Alexander Hollaender, who had been on the BEAR 1 Genetics Panel, and was in charge of the research of William and Liane Russell, both of whom were also strongly criticized by me. Hoffmann had strong historical ties to many other leaders in the genetics field, especially via his role as a staff officer at the NAS, assisting such scientists in their committee meetings, and as journal editor for Mutation Research, activities that involved developing and maintaining strong professional and personal relationships. As noted, my research was strongly critical of many of these people. Beyea should have felt obligated to share this information with *HPJ* to ensure that the reviewers would be aware of numerous conflicts and biases that Hoffmann brought to this process that could affect the peer review process. Based on the information presented, the COPE Retraction Guidelines indicate that the Beyea paper should be retracted.

Had I used any information from Hoffmann along these lines, I would have disclosed it as a personal communication from him. I did not do so and Calabrese does not suggest I did use anything personal about these people. I base my work on traceable documents.

I cited Hoffmann in my 2025 paper as part of his 2023 publication with me about statistics of thresholds and for reviewing drafts. In addition to getting excellent editorial suggestions from him, I counted on him, as a geneticist, to look for any incorrect use of language about genetics.

As in the case with George Hoffmann, Beyea failed to identify the interactions that one of his consultants, Kevin D. Crowley, has had with me concerning the historical foundations of cancer risk assessment and the LNT issue. In 2014, Crowley co-authored a letter to the editor with the President of the US NAS, Ralph J. Cicerone, which was highly critical of several of my publications (Cicerone & Crowley 2014). I rebutted the accusations of Cicerone, and Crowley in a follow-up letter to the editor (Calabrese 2014). These were highly contested letters. A year later Crowley again co-authored a letter of rebuttal to a different article (Calabrese and O'Connor 2014), again on the issue of LNT with the clear focus on our criticism of the US NAS BEIR VII report (Crowley et al. 2015). Crowley is the Director of the Nuclear and Radiation Studies Board of the US National Research Council, a high-level position within the US NAS. Thus, one can readily see how Crowley could have co-authored the letter to the editor with the President of the NAS criticizing me if the issues I had raised were of sufficient seriousness. Crowley was also the NAS Project officer in charge of the assessment of the Fukushima Nuclear Accident. In this context, Beyea was a member of that committee with Crowley.

Kevin Crowley, a superb editor, retired from NAS in 2017.

Crowley retired in 2017 and has no position within US NAS.

Beyea had an ethical obligation to inform the *HPJ* of the adversarial relationship that Crowley and I have had for the past decade, with it reaching to the heights of the President of the NAS, no small involvement. Since Crowley holds a position of considerable prestige within the radiation community, readers of the Beyea article would likely have the impression Beyea obtained guidance from a highly objective source in the US NAS. The

Calabrese has an adversarial relation with so many people that it would be hard to list all of their interactions with me that may have had some hidden influence on me. If Calabrese can make up new definitions of conflict of interest, I presume I can do as well. He fails to disclose in his articles denigrating Hermann Muller that he advocates and lobbies for a theory that competes with Muller's. How unethical to fail to disclose, I could say.

No, Crowley does not work for McNutt. Crowley retired in 2017.

This hostility towards Calabrese came from committee members, not NAS leadership. I disclosed in the article my institutional allegiance to NAS, having served on many studies of the NAS.

As I have said, there must have been two video series. Now that I have a raw transcript, I can show who saw correctly.

A technique that Calabrese uses is to demand something that no institution is going to accept. He then implies that by not complying they admit there is something to hide. He did this with his asking for a retraction by Science *of an NAS report that the journal reprinted in 1956. He does this here by asking a journal to give him special treatment by giving him summaries of reviewer comments.*

Is this a privilege that Calabrese thinks should be extended to anyone criticized in an article, or only to him? I would be interested in seeing a digest of the reviews he got from the papers of his that I have critiqued, along with his responses, but that is only allowed at those few journals that have public peer review.

readership needed to know that Crowley and I strongly disagreed on many issues related to the LNT story. This level of transparency is essential for an author such as Beyea to provide and for a journal to ensure, neither of which occurred. It is also important to note that I have continued my debate and challenges to Marcia McNutt, the current President of the NAS, for whom Crowley works. My dispute with McNutt was given an entire episode (i.e., Episode 14) of the HPS Documentary. This should also have been acknowledged by Beyea but wasn't because it is directly connected to the Crowley involvement. As noted earlier in the present paper, a recent NAS radiation committee explicitly refused to cite my LNT publications or to use the hormesis term in their report, each an attack on me and my work, showing the endemic hostility of the NAS leadership to my research on the LNT area and the sensitivity of that organization to my criticisms. Thus, Beyea again failed to provide the necessary transparency. This is a serious matter as it involves serious ethical failures by Beyea as manifested in his *HPJ* publication.

JOURNAL TRANSPARENCY: A BASIS FOR HONESTY AND FAIRNESS

It is apparent that the peer review process of the Beyea manuscript was seriously flawed and highly biased based on the number of deficiencies/errors, basic methodological inadequacies (e.g., reliance of only secondary source materials), as well as the many examples that strongly suggest that Beyea could not have watched multiple episodes of the HPS documentary due to numerous glaring errors (see a full documentation of this point in the Cardarelli paper), some of which the present paper identified.

This issue speaks to journal and editor integrity. I assume that journals and editors may defend the position that the peer review process cannot be violated under any circumstances. I support identity protec- tion of the reviewers, but not what they wrote. One can decouple what was written from who wrote it. For example, reviewer comments in the present case could be briefly summarized by a journal editor. In such a situation, there would be no ethical reason not to share the summarized information minus the identification of the reviewer.

Given the above concerns, it is necessary to see how the reviewers evaluated this paper. Assuming two reviewers, did both positively evaluate the Beyea paper, recommend acceptance and justify their respective judgments? Did they offer substantive criticisms? If so, what were the nature of these criticisms? Did either recommend that the paper be rejected? If there was a negative review, what was the basis of the rejection? How was this dealt

with by the Associate Editor? This allows the reader to more fairly judge the actions of the reviewers and the Associate Editor. Did reviewers come to the same independent conclusion as Cardarelli and me; that is, that Beyea most likely did not watch the entire documentary as well as multiple episodes while commenting on them. If no reviewer brought this issue forward, this would also create a specific concern for the quality of their reviews. Given the criticisms directed at the Beyea paper herein and the documented complementary paper by Cardarelli, it is necessary that the journal editor display adequate transparency as described above to restore damaged journal credibility. A final point relates to new ethical concerns. Since Beyea claims to have watched the entire documentary, yet overwhelming evidence suggests otherwise, it raises ethical violation concerns that the Editor needs to address within the context of retraction of the Beyea 2024 paper.

I ask, is sharing reviews with non-authors something that Calabrese would have provided when he was editor of Dose-Response? Would he recommend such a policy for that journal now, where he is still Editor Emirates?

Actually, I was careful to not comment directly on the documentary, because there was no transcript that I could use to hold Calabrese fully accountable. In my 2025 paper, I commented only on what Cardarelli et al. wrote and what Calabrese had written in his published papers.

The absence of an official transcript is what I call a lack of transparency. For this reply, after claims that I got the videos series wrong, I produced out of necessity a raw transcript using up-to-date software

FINAL STATEMENT

Nearly every conclusion offered by Beyea is inadequately researched and is often found incorrect or misleading, which is unfortunately reinforced and enabled by his failure to perform professional historical research. The Beyea article is plagued with instances of providing the reader with unreliable information concerning both the HPS documentary and my publications. The infor- mation is so erroneous that it seems quite apparent that Beyea could not have even watched large segments of the documentary. He also fails to cite key papers of mine that contradict many of his inaccuracies. The big picture is that Beyea does not have a good understanding of the topic on which he wrote, and it shows in dozens of ways as demonstrated here. In addition to his large number of mistakes, massive potpourri of misinformation, and substandard research methods, Beyea's assessment represents a serious misrepresentation of my research record and its contributions to the field. Instead of recognizing the value of the substantial number of historical discoveries that had remained hidden for nearly a century, Beyea brushes them aside, and directs an attack on my character and career efforts, lacking objectivity and reasonable fairness. Beyea also displayed profound ethical failings when he did not provide proper disclosure information about potential strong biases against me by one of his identified consultant experts. The Beyea story is not unique but represents another in a rather long line of ideologically motivated publications.

That Beyea's manuscript passed the peer review of the HPJ is more than a serious warning sign to the edito- rial leadership, the society membership, and the publisher.

Calabrese remains tenacious to the end. However, he made no attempt to rebut his overlooking of language in the 1956 genetics report that acknowledged the great uncertainty in the illustrative calculations of total mutants -- acknowledgements that voiced the concerns of those who had written earlier about them in letters to chairman Weaver. Note that the caveats included in the 1956 report were listed in the Introduction in the first item discussed and in fuller detail in Footnote 23.

The most that the 1956 report could conclude was that the results of the calculations were very sobering and to some geneticists very serious. It is not surprising, then, that these calculations were not used for any recommendation of the committee. Their recommended allowable dose to the public was based on background radiation. They recommended that cumulative dose to the public not exceed 10 R in the first 30 years of life, which allows no more than a doubling of yearly background dose rate.

Calabrese's major charge of scientific misconduct fails because he overlooked the disclosures written in the final document, which had been written to allow committee members to sign off on it -- standard NAS practice with non-consensus material.

Beyea continues:

In addition, Calabrese made no attempt to rebut the occurrence in the 1956 genetics report that Neel's A-bomb genetic data was used in the report (to estimate a limit on the human doubling dose). Nor did he account for Neel's views being incorporated in the report through the sentence that some geneticists objected to the calculations because "... they recognize that there is a risk involved in extrapolating from mouse and Drosophila data to the human case." This too was a major charge by Calabrese of misconduct on the part of the 1956 Committee.

Calabrese does not rebut the fact as I wrote that "The 1956 Committee accepted the linear no-threshold (LNT) model for mutations at the start of their first meeting in 1955 and never wavered," In fact, the LNT principle was in the agenda of the first meeting. Thus, he does not rebut "... the idea that Warren Weaver's promise of potential funding from the Rockefeller Foundation, made in 1956 at the second meeting, influenced support for the LNT is inconsistent with the timing."

Instead of dealing with these and other overlookings of key text, Calabrese distracts, diverts and repeats his work as if it were not challenged.

He says I have not performed professional historical research. Calabrese, an amateur, is not a judge of professionalism. He cannot even get straight what is a primary document. I, too, am an amateur science historian, but I at least have a named science historian review my work for terminology and other concerns. Calabrese challenges historians, denigrating their work (e.g., Jolly's) when it serves his purpose. Calabrese is plagued with selective stakeholder perception, a charge that he does not directly try to contradict in this reply. What good are letters if you can't interpret them correctly, e.g., by judging them in chronological order?

It is Calabrese who has stated that Calabrese made a contribution to the field. No historian has done so. And I have argued that he has provided misinformation about misconduct.

I have made no attacks on Calabrese's character.

Beyea continues*: Attacking character is one of Calabrese's signature methods. He is good at it, I concede. He shows an example by going after my character in this paragraph, putting me in good company by accusing me of ethical failings.*

Another typical move shown here is to claim that those differing with him are ideologically motivated, but he is not so motivated. Everyone is against Calabrese, he tells us in his rebuttal, and he apparently contributed to none of it.

Calabrese, in his 44 journal pages of self-promotion. distraction, diversion, and error, has pointed out minimal flaws in my critique of his claims of scientific misconduct. Readers are not going to go through 44 pages waiting to see if he will respond to my charges about overlooking text. Clever politics, poor science.

Real unfairness from my critique? Calabrese gets a video series, an endorsing article in HPJ and a 44-page, self-promotional rebuttal to which I am barred from responding in the journal. As a result, few readers will ever know that he has spoken untruthfully about my source, hid behind those untruths, and has avoided dealing in rebuttal with the text he overlooked in key documents.

Calabrese portrays himself as a paragon of virtue, surrounded by enemies, all the while he denigrates historical figures and attacks their character based on false premises. He complains that my paper will continue to exist, but cares not a whit about all his papers that continue to exist and negatively influence search engines and AI about past researchers who he falsely denigrates. Double standard. Calabrese can criticize but cannot be criticized.

It is Calabrese who testifies before Congress. Calabrese's work is as political, as is the work of all who argue publicly for regulatory change, either way.

I show in the introduction that Calabrese in fact in episode 22, as in his paper on the subject, argues for a threshold/hormesis dose response below a certain dose, claiming that a linear response above the critical point retains the LNT. It is disingenuous to claim he is not calling for the elimination of LNT.

What is their accountability? What was the nature of the peer reviews and how were they acted upon? As one can see from my assessment, the published Beyea paper is terribly flawed, often not providing reliable information. Did the reviewers simply miss the numerous flaws that I documented?

In this specific case, the Editor-in-Chief has indicated that he relied entirely on the decisions of a designated Associate Editor, without any oversight, review, or knowledge of the selection of the reviewers, comments of the peer reviewers, and how they were handled. Yet, the editor-in-chief is supposed to ensure a fair and professional process. So, how did the Editor-in-Chief ensure this since he did not have any information as to what occurred? Based on subsequent discussions, he indicated that "fairness" is ensured by permitting me to respond with the present Commentary article to the Beyea paper. Within this context, real unfairness becomes apparent when it is realized that the editors failed to notify me of the accepted Beyea paper so that I could offer possible comment at the same time as the Beyea paper. I learned about the acceptance of the Beyea paper nearly 4 mo after it was accepted and online via my own search. A major flaw with the editor's fairness "policy" is its inherent unfairness in that the Beyea paper will continue to exist and will be used for political purposes even if I am successful in my rebuttal and show that almost all of what he asserted was not justified and not reliable. In fact, opponents of the documentary who serve on the HPS Board of Directors demanded that the three articles of Shrader-Frechette — with my name unceremoniously placed in the titles -- be posted with the documentary on the HPS website, a demand that was not approved even though her articles had no linkage to the history of cancer risk assessment. This is a sign of the nature of the politicized process. Thus, these papers, and the now published Beyea Commentary, even if largely discredited, can have a very long functional societal half-life and can get recycled, as in the present situation.

This is a good example of why the nature of peer review demands both fairness and high professional competence, including necessary fact checking, as it serves as a critical check on such political activism and its infiltration into the scientific process, while ensuring accuracy and that the information provided is reliable. For example, the *HPJ* reviewers should have been very knowledgeable of the documentary and should have watched it again (all 10 hours!) during the peer review process in order to provide a more informed peer review. If they had done so, they would have known that Beyea's allegation that I argued for the elimination of LNT was false based on watching episode 22. However, it seems that none of the reviewers knew this fundamental issue, allowing the false Beyea statement to go unchallenged. The same is the case

for his inaccurate statement that I claimed that the BEIR I Committee did not consider the issue of dose-rate. In this case as well, it is quite clear that Beyea could not have watched episodes 19-21 nor have read my relevant publications, which discredited his statements. Thus, the decision of the Editor-in-Chief is important because it prevented me from challenging the Beyea paper in real time, delaying my response by possibly 10 mo, and perhaps longer, thereby allowing Beyea to get his evaluation more solidified in the minds of readers before it is challenged. The long delay might also make readers wonder why I have not responded, perhaps now incorrectly thinking that Beyea may be correct. It represents a significant "silent editorial strategy" that ensures a profound lack of fairness. Yet, the Journal claims to follow the ethical guidelines of COPE.

It is true that I gave the wrong reason for why Calabrese accused the 1972 NAS committee of falsifying the record, i.e. scientific misconduct. My apology. Nevertheless, Calabrese was wrong in his rebuttal to say that he never accused the 1972 committee of misconduct. He must have forgotten his allegations.

Had I given the correct reason, I don't think the reader's impression would have changed.

The Editor-in-Chief remarkably claims that this is how the *HPJ* is being fair: BUT fair to whom? To most fair-minded individuals, this gives an undeserved advantage to Beyea and is harmful to the field as well as to me. Further, I have been informed that my paper would undergo the *HPJ* version of peer-review, a process that I have shown in this paper to be unfair, largely uninformed, and untrustworthy. Who would be the reviewers? If they are the same people who advocated/supported publication of the Beyea paper, then my paper would be almost as critical of them as it is of the Beyea paper. This situation would then place my manuscript within yet another unfair and severely compromised setting. Yet, these decisions are hidden from the author and the readership, again an unfair situation. This entire current process needs proper transparency, showing the originally submitted manuscript of Beyea, summarized reviewer comments, and what changes were made — the full process. If no or very few changes were made in the Beyea review, then what does that mean both for how the Beyea paper was treated and for my paper?

As I have said, Calabrese gets a video series, an endorsing article in HPJ and a 44 page, self-promotional rebuttal to which I am barred from responding in the journal. As a result, few readers will ever know that he has spoken untruthfully about my sources, hid behind those untruths, and has avoided dealing in rebuttal with the text he overlooked in key documents.

Fairness and responsibility demand that such papers need to be evaluated fairly by knowledgeable and unbiased peer reviewers. This is the ethical obligation and responsibility of the Editor-in-Chief, his designated associate editor, and the publisher — an obligation that was not fulfilled in this case, with each having a failed responsibility. This also appears to have occurred in the publications of the Shrader-Frechette (2010) and Elliott (2006) papers for other journals and editors.

Calabrese just can't get a fair shake with journal publications who act unethically in his case by publishing unfair critiques. Only Calabrese is ethical.

Scientific societies are valued because of their commitment to objectivity, accuracy, and fairness, a standard that was not achieved in this case for any of these three areas at the *HPJ.* In fact, the number of serious errors and omissions in the Beyea paper are far beyond repair even with the publication of the present paper, which has tried to correct the record. There are still numerous other

Objectivity, accuracy, and fairness. These are the very qualities that I challenge Calabrese on.

Calabrese in rebuttal has avoided responding to the charge of overlooking key test. He has no direct defense, presumably trying to imply that his own selected examples of correspondence and his own selective interpretation of transcripts make it unnecessary for him to worry about contradictory information that is actually written in the reports and articles that are the end products of the processes. For instance, it apparently doesn't matter to Calabrese that Weaver accounted for the complaints and worries in the final 1956 NAS report that were expressed during the process by some members.

errors that remain to be corrected. All major scientific journal publishers have retraction policies. The first priority of these publishers is to ensure that the information is reliable. In fact, the first priority according to COPE for retraction is the following: "…clear evidence that the findings are unreliable." Knowing that this was the chief criteria of COPE for retracting, each area of this paper that identified unreliable findings of Beyea were noted as unreliable. The COPE criteria demand retraction. Not to do so by the Journal would compromise the very ethics that the Journal claims to uphold.

Given this situation, the *HPJ* has a clear obligation to retract the Beyea paper, in order to inform the reader of excessive unreliable information. The journal failed in its evaluation of the Beyea paper. It now needs to correct that error by retracting the Beyea paper. In addition to the issue of providing copious unreliable information, Beyea's actions have violated the COPE Retraction Guidelines as noted above, for "undisclosed conflicts of interest that could bias interpretation of the work or recommendations by peer reviewers." This provides yet another documented basis for *HPJ* to retract the Beyea paper. Failure to retract the Beyea paper would represent an abject failing of the *HPJ* leadership and further compromise the standing of the Journal.

CONFLICTS OF INTEREST AND SOURCES OF FUNDING

No conflicts of interest or sources of funding are declared.

DATA AVAILABILITY

No data were used for the research described in the article.

USE OF GENERATIVE AI AND AI-ASSISTED TECHNOLOGIES

No AI software was used in the preparation of this manuscript.

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Errata for:

"False and misleading claims of scientific misconduct in early research into radiation dose-response: Part 1. overlooking key historical text"

Health Physics 128: 507–523; 2025.[1]

which is available here.

Draft Version 1.0. 31 May 2026.

Reasons that Calabrese gave for saying the 1972 NAS Committee committed scientific misconduct.

In my 2025 paper, I implied that Calabrese has claimed scientific misconduct on the part of the National Academies committee that authored the 1972 BEIR report. I gave the wrong reasons for my allegation, which I here correct. Calabrese did not say the Committee ignored the dose rate dependency of genetic mouse data. I should have said that Calabrese claimed the Committee ignored the female mice data." [2, 3] He was wrong because the Committee included

[1] https://journals.lww.com/health-physics/fulltext/2025/06000/false_and_misleading_claims_of_scientific.9.aspx

[2] "Nonetheless, the BEIR NAS Committee retained the LNT model, **relying only on the male mice data**."Calabrese EJ. Troubled History of Cancer Risk Assessment. 2019b.. Also: "They decided to construct a linear dose response from the lowest dose tested (i.e., dose associated with the ~70% decrease in mutations in males) to the origin. Calabrese EJ. The linear No-Threshold (LNT) dose response model: A comprehensive assessment of its historical and scientific foundations. Available at: https://www.sciencedirect.com/science/article/pii/S0009279718311177. Accessed 13 June 2024. Chemico-Biological Interactions 301: 6–25; 2019a..

[3] "Furthermore**, the decision to base the LNT recommendation on the male mouse data of Russell** is now seen as flawed (Calabrese, 2017a,b), providing no support for the BEIR (1972) decision in favor of LNT." Calabrese EJ. The Muller-Neel dispute and the fate of cancer risk assessment. Environ Res 190: 109961: 1–11; 2020. p 10

the female data (approximate zero response) in their average over sexes.[4] A sex average was an appropriate choice for application to the general population. Calabrese went further in his criticism, saying the committee *avoided*.[5,] and *rejected*[6,] the use of the Russells' female mice data, which suggests to me a charge of falsification of the research record, i.e., scientific misconduct. If that was not enough, Calabrese also charged the 1972 BEIR I committee with misleading the scientific community[7] and deliberate misrepresentation of the facts[8] about its

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[4] "5. If the sexes differ in sensitivity, use the unweighted average of data for the two sexes." At page 51 of NAS. Effects on populations of exposure to low levels of ionizing radiation. Available at: https://nap.nationalacademies.org/catalog/18994/effects-on-populations-of-exposure-to-low-levels-of-ionizing-radiation. Accessed 13 June 2024. Advisory Committee on the Biological Effects of Ionizing Radiation, Division of Medical Sciences; National Research Council, National Academy of Sciences; 1972.. This is what the committee did with the mouse data on p. 52: "A chronic radiation dose to mouse spermatogonia produces about 0.5 x 10·7 recessive mutations per rem per gene. The reproductive cells of the female, for most of their lifetime, are very much less mutable than those in the male, even from acute irradiation… Therefore, we take the average of the two sexes as 0.25 x 10·7… "

[5]" It did so while **avoiding** the use of the Russells' female mice data that demonstrated a striking threshold effect (at 27,000-fold greater than background) and defaulting to the male mouse data that suggested (but did not quite achieve) a threshold, thereby preserving the LNT mantra of the BEAR I Genetics Panel. Calabrese EJ. Linear non-threshold (LNT) fails numerous toxicological stress tests: Implications for continued policy use. Chemico-Biological Interactions 365: 110064; 2022..

[6] "Therefore, the BEIR I Genetics Committee, with W.L. Russell as a member, selected Russell's male mouse data, **rejecting** his female mouse mutagenicity findings that had displayed an exposure threshold 27,000 times background."Calabrese EJ. The Muller-Neel dispute and the fate of cancer risk assessment. Environ Res 190: 109961: 1–11; 2020..

[7] "**BEIR I misleads the scientific community** with regard to BEAR I ignoring human genetic studies." He goes on to say, "This seems to be an outstanding deception for which there is no supportive data, only much data showing that there was no such review. Furthermore, with a 16-year period of historical hindsight and the availability of the multiple Neel and Schull publications, this statement by the BEIR I Committee (1972) is **extremely misleading, if not patently false**."Ibid.. Misleading the scientific community would be falsifying the scientific record.

[8] "For example, in 1972 the BEIR I Genetics committee stated that the BEAR I Genetics Panel based its recommendations principally on Drosophila because there were no adequate human data available at that time. **This statement is a deliberate misrepresentation of the facts.**" Calabrese EJ. Ethical failings: The

predecessor committee, the 1956 NAS genetics committee in connection with the Neel A-bomb data. Misleading readers and deliberately misrepresenting the scientific record certainly qualify as scientific misconduct. Yet, Calabrese in rebuttal claims he never accused the 1972 Committee of scientific misconduct:

> "This statement by Beyea is alleging that I accused the BEIR I Committee (1972) of scientific misconduct. He provides no evidence to support such an allegation. While it is true that I made such an allegation concerning the 1956 BEAR I Genetics Panel, this was never the case for the 1972 BEIR 1 Committee." (Calabrese 9900) at 14th page.

He never actually uses the words, scientific misconduct, but his words are equivalent.[9] I attribute his denial to bad memory, which he acknowledged in Episode 12 of the video series.[10] Also, I should have provided the citations that I provided here.in order that Calabrese could have contested my claims and others could have checked my citations. Nevertheless, my allegation was correct despite my giving the wrong reason in Part 1.[11]

Words left out about total mutants.

In the summary, I left out words in a sentence about the calculation of total mutants, thereby misreporting the main text, although the point was still correct, namely that there were two types of calculations presented. I should have included on p. 519 the words shown next in bold

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problematic history of cancer risk assessment. Environ Res 193: 110582; 2021.. A deliberate misrepresentation of the facts would be falsification of the record.

[9] Cardarelli in his rebuttal makes a big point that Calabrese defines scientific misconduct as falsification of the record, showing an excerpt on screen from the National Academy of Sciences. There are lesser degrees of scientific misconduct. For instance, self-plagiarism is sometimes considered scientific misconduct.

[10] "In episode 12 after 13 minutes, Calabrese says, "I'm reading this, and you know, my memory isn't the greatest in the world sometimes."

[11] As I discussed in the introduction, Calabrese also accuses members of the committee separately as having committed scientific misconduct.

format: "… the two low estimates only covered first-generation **tangible** damage, unlike the others that went out to 10 generations **and beyond**." I thank Calabrese for pointing out this editing problem in the Summary.

I next include two corrections to Part 1 that I had attached to Part 2:

"Corrections--I take this opportunity to make two corrections to Part 1 of this series.[12] First, I stated without a date limitation, unfortunately, that Dr. Calabrese had not published his historical claims about radiation in radiation journals. Subsequent to the cutoff in 2024 of publications I had set for my Part 1 Forum article, Calabrese published a historical article in *Health Physics* about Ernest Sternglass and Hermann Muller (Calabrese and Giordano 2024), where many of Calabrese's standard historical claims were repeated by reference. As for a 2022 article in *Health Physics* by these same authors (Calabrese and Giordano 2022), I did not consider it an article requiring review by an expert in ionizing radiation epidemiology. It focused on a mistake made in one out of eight versions of a statement found in an NAS condensation of six simultaneous 1956 reports prepared by a science writer. Review was by committee chairs, not the 103 committee members, as is standard practice to this day for major reports at the NAS when trying to translate for the press and public. However, the article did appear in *Health Physics,* a radiation journal.

[12] These two corrections to Part 1 were made prior to the publication of the ahead-of-print versions of the rebuttals by Calabrese and Cardarelli.

Second correction: In Part 1, it is stated: “By 1948, Muller would drop the ‘we believe’ caveat in a presentation he made trying to convince doctors to reduce high doses in medical practice (Muller 1948), but this was 2 years after his Nobel Prize address.” (Beyea 2025). In fact, Muller’s presentation was given in April of 1947, some 4 months after his Nobel Prize address. I have no way of knowing when the manuscript was actually submitted to the publication, but a value of 2 years is incorrect.”